THURSDAY, OCTOBER 24, 1895.

THE METALLURGY OF IRON.

The Metallurgy of Iron and Steel. By Thomas Turner, Associate of the Royal School of Mines. Vol. i. "The Metallurgy of Iron." (London: Charles Griffin and Co., Limited, 1895.)

THIS is the third volume of a valuable series of treatises on metallurgy, written by Associates of the Royal School of Mines, under the able editorship of Prof. Roberts-Austen. It occupies an intermediate position between a text-book and an exhaustive treatise, and is intended not only for the use of the student, but also of persons who are connected with the manufacture of iron and steel, and who, therefore, may be assumed to have already some knowledge of the subjects discussed.

The attempt made by the author to compress within the space of 367 pages a useful account of this vast subject—the metallurgy of iron—has been satisfactorily accomplished; and although in some of the chapters the condensation is perhaps unduly great, yet this fault is minimised by the numerous references, which abound in the text, to original papers where full details may be found. In preparing these references, the author appears to have made a painstaking research into the literature of the entire subject, and this, together with his practical knowledge of its chief branches, has resulted in the production of a valuable treatise, which covers the whole field of the metallurgy of iron more completely than any other book in our language. As a standard of reference for detailed information, the *Journal* of the Iron and Steel Institute has been wisely chosen, as in it all advances in the metallurgy of the metal are recorded, and the more important are dealt with by specialists of note; it is, besides, easily accessible.

The volume begins with a patiently compiled summary of the history of iron, in which the origin and development of the metallurgical processes for the production and purification of the metal, and of the furnaces and appliances used, are clearly traced from the earliest times up to the present day.

A condensed *résumé* of the nature, composition and characteristics of the chief iron ores, and of the modes of preparing them for smelting, follows in chapters iv. and v. In a future edition the latter chapter might be extended with advantage, for, although no important methods are omitted, the descriptions of some are very brief.

The next five chapters (vi., vii., viii., ix. and x.) deal respectively with the blast furnace, the blast, the reactions which occur in smelting, the fuels used, and slags and fluxes. The general arrangement of a blast furnace plant is illustrated by sketch plans of a modern Cleveland and American (Edgar Thomson) works, and under "Construction of the Blast Furnace" a typical furnace of each of these works is selected for detailed description. The marked differences which are found in the internal lines and dimensions of the furnaces of the two countries, and in their practical working, are compared, and the reasons which have been advanced in favour of each are clearly stated and discussed; all of which tend to demonstrate that there can be no universal standard form, size, or method of working for a blast furnace. There are, however, undoubtedly some points in American practice which might be adopted with advantage in this country.

The diagram given on p. 127, illustrating the application of the recording pyrometer, as devised by Prof. Roberts-Austen, for the measurement of the temperature of the hot blast, is instructive, and shows conclusively the value of this instrument to the blast furnace manager.

The reactions which take place in the blast furnace, and the conditions which regulate the consumption of fuel, are very fully considered. Here the editor has allowed the author to state his own view of the theory of reduction, probably because it is evidently a "theory." It differs from that which Prof. Roberts-Austen is known to teach in his lectures at the Royal School of Mines. In chapters xi. and xii. the "Properties of Cast Iron" and "Foundry Practice" are discussed with a thorough knowledge of the subjects, both chapters being full of important matter. The effects of the presence of other elements, especially of silicon, on the physical characters of cast iron, are ably and comprehensively set forth, and experimental data of much value to the practical founder are given in demonstration of the relations which exist between the chemical composition of the metal and its fitness for special purposes. The necessity for a knowledge also of the relations between its hardness and strength is wisely insisted on, as, when these are fully grasped, the iron-founder requires only the information how to harden or soften his metal at will by the use of silicon or other agents, to produce castings in which the crushing, transverse and tensile strength, or othe characters, shall predominate as desired. These chapters deserve the careful study not only of the student, but also of the practical man, if he wishes to work intelligently, and so avoid the uncertain results which follow the "rule-of-thumb" methods still too often practised in our foundries. In no other text-book are the subjects of these chapters so lucidly and completely treated.

A description of the methods for the "Direct Production" of wrought iron—the subject of numerous modern patents, and of probably more failures—follows; and the three next chapters (xiv., xv. and xvi.) deal with the "Indirect Production" of the metal. Of these, the chapter devoted to "Puddling" is one of the best in the book. The account of the process and its various modifications it contains is worthy of high commendation. The concise descriptions and explanations which are given, many of which are based on the author's personal experience and investigations, and the useful practical suggestions which abound regarding the relative economy and extent of purification resulting from modifications in the method of conducting the process, cannot fail to be of great value to all iron-workers.

The corrosion of iron, a subject of not a little importance when we consider the disastrous results which may arise from the oxidation of a boiler-plate, a girder, a rivet, or a wire rope, is reserved for the last chapter of the book. The conditions under which this change occurs, the methods which are adopted for preventing or retarding it, and the experimental data on which these are founded, are carefully summarised here.

The book, however, is too good to be dismissed with commendation alone, and it would be unfair to its author and readers if we omitted to indicate one or two points in which its value may be increased in a future edition, which will doubtless be soon required. The illustrations are a weak feature of the book; several are unsatisfactory, being either rough in execution, wanting in detail, or too small in size, and a few can serve no useful purpose. We are sure the student would be grateful for the improvement of some, the omission of others, and the substitution for them of working drawings, not diagrams. We trust the author will bear this in mind in the preparation of his companion volume on steel.

The other faults are few and of a minor character. They are chiefly those of excessive condensation in the sections dealing with the blast furnace. These sections might be usefully expanded by the insertion of additional details respecting the actual erection of a furnace; also of an example of actual working similar to the excellent *résumé* given of the process of puddling.

The book, however, is an excellent one, thoroughly up to date, and a welcome addition to modern metallurgical literature. We can confidently recommend it to metallurgical students and all concerned with the manufacture and use of iron.

W. GOWLAND.

THE LIFE OF RENNELL.

Major James Rennell and the Rise of Modern English Geography. By Clements R. Markham, C.B., F.R.S. (The Century Science Series.) (London: Cassell and Co., 1895.)

"JAMES RENNELL was the greatest geographer that Great Britain has yet produced." This, the first sentence of the preface, is the text of the biography. The authority of the President of the Royal Geographical Society, himself the leading geographer of the day in this country, may be accepted as sufficient evidence of Rennell's pre-eminence. The name would perhaps not suggest itself to one who had a less thorough knowledge of the rise of modern English geography; for until the publication of this little volume, Rennell was without any more pretentious memorial than an obituary notice or a paragraph in a biographical dictionary. Mr. Markham writes with an enthusiastic singleness of aim; intent on illustrating his theme, he has perhaps on one or two occasions failed to criticise his own conclusions very severely before accepting them. Possibly he may unconsciously have applied the method *post hoc ergo propter hoc* in connecting all British progress in geography during the last fifty years with a name which cannot be said to be familiar even amongst professed geographers. Indeed we believe that this happily-timed biography will make Rennell's example more fruitful in results in the next few years than it has been during the sixty-five which have elapsed since the death of the great geographer.

The time is appropriate, for the recent meeting of the International Geographical Congress in London has brought into public notice the superiority of other nations in the organised study of geography as a branch of science definite and distinct from others, capable of being cultivated by research and of being applied to numberless practical purposes

Mr. Markham repudiates the suggestion that Major Rennell was an "arm-chair geographer"; but we are not sure that this somewhat hackneyed term is necessarily one of reproach. Rennell was greatest as a student and a critic, and by the practical experience of his earlier life he fitted himself to speak *ex cathedrâ* on questions, where insight and judgment were required to interpret, even to the travellers themselves, the full meaning and importance of their journeys. A professor's chair would have been his true place.

The greatness of Major Rennell may best be understood by a glance at the mileposts of his life. He was born in 1742, at Chudleigh, in Devon, and at the age of fourteen he joined the Navy, where he saw some service and learned to survey. In 1760 he went out to India as a midshipman; but after three years' hard work, largely occupied in surveying in the Indian Ocean, he left the Navy, joined the East India Company's service, and received the command of a ship. As if by a stroke of magic he was nominated Surveyor-General of Bengal and gazetted an ensign in the Bengal Engineers in 1764, when only twenty-one years of age. In this new and congenial sphere he worked devotedly for thirteen years, personally surveying the most unhealthy part of India with such success that in 1779 he published the "Bengal Atlas" containing the first authentic maps of the province. He left India in 1777, and, settling in London, devoted himself to critical geographical studies. His first purely geographical work was a "Memoir to the Map of Hindostan," and the map itself. In 1781 he became a Fellow of the Royal Society, and subsequently he communicated two papers to the *Philosophical Transactions*. Although ignorant of the classical languages, he studied the works of the Greek geographers in translations, and so produced his famous "Geography of Herodotus" and "Comparative Geography of Western Asia." Then turning to the burning question of his time in geography, the penetration of Africa, he pieced together the information brought home by Ledyard, Hornemann, Mungo Park, and other explorers sent out by the African Association. Here the results of subsequent discovery did not always confirm the provisional conclusions he arrived at from a critical study of the data at his disposal, but his controversies as to the course of the Niger interest the world no more.

Mr. Markham considers that Rennell was "the founder of another branch of the science of geography, which has since been called oceanography"; yet we find in Dr. Murray's compendious history of oceanography in the summary of the scientific results of the *Challenger* Expedition, a much more ancient lineage for that branch of science, and in the record of its development Rennell's name is not even mentioned. He certainly succeeded in calling attention to the importance of ocean currents, and made many shrewd observations as to their origin, preparing the way for the wider generalisations of Maury. He strongly held the theory that ocean currents are primarily due to the prevailing winds; and it is interesting to notice that the particular current issuing from the Bay of Biscay, to which his own name is attached, should only last year have been shown by Hautreux to have no permanent place, but to vary in force and direction with the changes of the wind.

It would be impossible to notice the numerous memoirs by which Major Rennell impressed the learned world of his time. With Sir Joseph Banks and other friends, he formed a sort of social circle for travellers and scientific men, which led to the formation of the Raleigh Club in 1827, and may be said to have formed the nucleus of the Royal Geographical Society established three years later.

Rennell's training was purely a practical one in the hard work which gave him a mastery of the technicalities of surveying and map-construction. Knowing the actual forms of sea and land at first hand, able himself to delineate them with exceptional skill, he could not make the mistakes which beset the merely theoretical student. This is still the one way to become a practical geographer, only in the present day a working knowledge of geology must be added to proficiency in the arts of observation and measurement. On such a foundation, so gained, theoretical instruction may profitably be superimposed. Mere lectures on theoretical geography, isolated lessons in the use of instruments, do not suffice to make a man a geographer, any more than lectures on theoretical chemistry and a few repetitions of the routine of simple analysis will make a man a chemist. If British geographers are to catch up and keep pace with those of the continent, they must receive systematic training in their student days, and take up geography as a serious study, as one takes up any other science. For, alas, the good old days are gone, and there is no Warren Hastings on the threshold of the twentieth century to confer pensions of £600 at the age of thirty-five on the would be Rennells of to-day! As geological students have to follow other methods than those of Murchison, so present-day geographers cannot take Rennell too literally as their model; and Mr. Markham plainly states that he looks to the labours of the University lecturers in geography to maintain the succession of British geographers. If this is to take place, there must be fresh organisation and encouragement of pure geographical research on the part of the Universities. Much progress is improbable as long as the antithesis between "geography" and "science" is a possible figure of speech. It is not so in Germany.

HUGH ROBERT MILL.

COUNTER-IRRITATION.

The Theory and Practice of Counter-Irritation. By H. Cameron Gillies, M.D. (London: Macmillan and Co., 1895.)

DR. GILLIES has selected a subject rich in literature but poor in experiment, and has treated it entirely from the literary as opposed to the experimental side. The first part of the book is devoted to a *résumé* of the literature of counter-irritation, and inflammation, which Dr. Gillies rightly considers he must not only quote, but criticise. Some of his criticisms we do not understand, some are entirely superfluous, Dr. Gillies taking up much space in demolishing theories which in the present day nobody could possibly believe in, some—and two of these we shall consider—show a want of scientific understanding.

On page 73, our attention is drawn to a paper by Dr. Hollis, published in the St. Bartholomew's Hospital Reports for 1874. Dr. Hollis showed that vesication could be produced in the Actiniæ by the local application of liquor ammoniæ. The importance of these researches consisted in the fact that they demonstrated that the living cell itself, using this term in its general sense, was capable of reacting to an irritant. It is to work done exactly on these lines by Metschnikoff[1] that we owe the modern theory of Phagocytosis. The physiology, the pharmacology, and the chemistry of the cell are presumably to Dr. Gillies, as "provoking" as he admits Dr. Hollis' monograph to be. The second class of experiments performed by Dr. Hollis demonstrated that local reaction to irritants took place in the excised tail of a newt, thus showing that this local reaction was independent of the general circulation. Dr. Gillies objects to "all such experiments, not only upon moral and humane grounds, but on the ground also that we have not been able to make sure that any good has come by them." "The tail is either dead or living, if living the result only shows that it is a living result; if dead we are not as physicians concerned with the chemistry of the action."

On page 78, our author considers an article by Dr. Lauder Brunton in the St. Bartholomew's (not the St. George's) Hospital Reports for 1875. Dr. Gillies differs from the author upon two points. First, he (Dr. Gillies) denies that inflammation can occur independently of congestion. One would have thought that this had been settled by Hollis. The discrepancy is explained when one finds, after a page's reading, what Dr. Gillies means by congestion—"an acceleration of the processes of nutrition." When arguing with a physiologist it is as well to adopt the usual physiological terminology. The second point of difference is Brunton's dictum that "pain in an inflamed part is probably due to distension of the vessels and pressure on the nerves." "The characteristic pains of neuralgia so called," says Dr. Gillies "are not easily if at all referable to the pressure from active congestion." Is a nerve which is the seat of neuralgia an inflamed part?

Dr. Gillies evidently believes that "he alone destroys who rebuilds," so we are not left merely amongst the ruins of other theories, but are provided with a "new" one. "Whatever good comes by the use of counter-irritants is because, by their irritant effects, they stimulate the activity of the tissues of the part to which they are applied and accelerate the blood supply thereto, so increasing nutrition or repair, as the need may be." This is the only new theory which we have been able to extract from chapter vii. What about the remote effects of counter-irritants? If Dr. Gillies is convinced that whether directly or remotely counter-irritants act beneficially only when they directly, or reflexly, increase the blood supply, that is at least a coherent theory; we think it quite probable that irritation of a given skin area by a blister or otherwise can give rise to reflex dilatation of the corresponding vascular area. Bradford[2] actually observed dilatation of the vessels of the kidney upon stimulating the central ends of the posterior roots of the so-called renal area, whereas stimulation of the central end of an intercostal nerve always caused contraction. Dilatation of the vessels of the splanchnic

[1] "Leçons sur le Pathologie comparée de l'inflammation."
[2] *Journal of Physiology*, vol. x. 404.

area has been observed upon stimulation of the central end of the sciatic nerve during chloral[1] and pyridin[2] poisoning, showing the influence exerted by the condition of the centre at the time of peripheral stimulation.

Of the second, the so-called "practical" part of the book, we have little to say. From what we have read, we regard Dr. Gillies' practice as no sounder than his theories. The reprint with which he provides us of Dr. Davies' original communication on blistering in acute rheumatism, and the controversy thereon, is the most interesting part of the book. We should like to know who it is who believes that the "serum" is "*abundantly* charged with lactic acid" in acute rheumatism; and, supposing it was, how much one is likely to get from the serum, say, of half a dozen blisters? (p. 88.) To sum up our remarks, we do not consider the book of value either to physicians or physiologists. The facts it contains are not new, and the theories do not justify their existence, since they fail to fulfil the conditions which should be demanded of all hypotheses, viz. to indicate lines of research which shall offer a reasonable hope of increasing our knowledge. One merit which it possesses, is that it may draw attention to some valuable pieces of work which might perhaps otherwise have been disregarded.

F. W. T.

A NEW DEPARTURE IN GEOMETRY.

Die Grundgebilde der ebenen Geometrie. By Dr. V. Eberhard, Professor at the University of Königsberg i.P. Bd. I. 8vo. xlviii. + 302 pp. Five plates. (Leipzig: Teubner, 1895.)

THE history of Analytical Geometry affords a curious subject of study to the thoughtful mathematician. It would seem that equations between coordinates were first used to express spatial relations discovered by intuitional processes, and the equations were combined algebraically to discover other implied spatial relations. For this purpose it was necessary to interpret in geometrical terms equations arrived at by algebraic processes from geometrical data, and the facility thus acquired led men to seek for similar interpretations of equations set down without reference to geometrical conditions. Hence it happens that modern developments of Analytical Geometry appear rather to present algebraic facts in geometrical language than to deduce results that can be apprehended by intuition from data of intuition. Such a notion as that of a cubic surface, for instance, would seem to be essentially analytical, and although it has been proved possible to arrange a geometrical construction for an algebraic curve whose equation is given, yet the construction arrived at is so artificial that intuition fails to grasp by its aid the necessary form of the curve. Looking at the subject in this way, it seems hardly too much to say that the algebra which was designed to be the servant of the geometer has become his master.

Some such reflections as these form the starting-point of Dr. Eberhard's work. The volume under notice is to be the first of a series, and in his long preface[3] he sets forth his aim and method. Here, after tracing the origin in experience of simple geometrical notions such as those of the straight line and the plane, he divides curves and surfaces into two classes, the regular (gesetzmässig) and the fortuitous (zufällig), and proceeds to inquire after intuitional criteria available for distinguishing between them. He defines a regular locus as one in which a relation that can be apprehended by intuition connects a variable point of the locus with a finite number of points fixed in it. The kind of relation which he admits as capable of being apprehended by intuition is essentially topographical. This will be elucidated by considering the example he gives. Let a system of points be taken, and let planes be drawn through them three by three. These planes will in general intersect in other points besides those of the original system. Let planes be now drawn through the points of the extended system three by three. These planes will again intersect in some new points, and the process can be continued. Let the process be arrested at any stage, and suppose a set of four points of the extended system lie in one plane. If one of the points of the original system were slightly displaced these four points would generally cease to lie in one plane, but if the particular point of the original system were displaced on a certain surface, the four points would remain in a plane. This property constitutes a definition of the surface available for intuitional geometry. It will be seen from the example that the method rests upon the topographical relations of systems of points.

The description of these relations for a given system can be carried out systematically, and the process consists in the use of two related notions. The first is the notion of "characteristics," and the second is the notion of the "index" of a point in a plane system. If three points out of four are taken in a definite order, the triangle formed by them is described in the positive or negative sense by an observer on the same side of their plane as the fourth point. The sense of description of the triangle formed by three points in a definite order for an observer on a definite side of their plane is the characteristic of the three. The index of a point in a plane system is the order in which a line turning about that point meets the other points of the system. A statement of the indices simplifies the problem of stating the characteristics.

The bulk of the present volume is taken up with theorems concerning the characteristics and index-systems of groups of points in a plane, and they are fully exemplified in the cases of groups of four, five, and six points. In an investigation of so novel a character we find, as we might expect, original methods of working and difficult arguments. The want of figures in illustration of the earlier chapters, and some of the notations employed, combine with the nature of the subject to render the book difficult to read.

The endeavour to make the geometry of curves and surfaces of high degrees more intuitive is laudable, a new classification of loci founded on geometric rather than algebraic principles is also a worthy object of research, and the idea of grounding such a classification in topographical circumstances is ingenious; but a final judgment as to Dr. Eberhard's success in these directions can only be pronounced after his complete work has been given to the world.

A. E. H. L.

[1] Foster's "Physiology," 4th edition, p. 205.
[2] Brunton and Tunnicliffe, *Journal of Physiology*, xvii. p. 272.
[3] The first twenty-nine pages of this preface are separately published as tract, with the title "Über die Grundlagen und Ziele der Raumlehre."

OUR BOOK SHELF.

Handbook of Grasses; treating of their Structure, Classification, Geographical Distribution, and Uses, also describing the British Species and their Habitats. By William Hutchinson. 8vo. Pp. 92, 40 woodcuts. (London: Swan Sonnenschein and Co. New York: Macmillan and Co., 1895.)

THIS is a cheap popular work, adapted for the use of elementary students. There is nothing that covers the same field in existence already, and it fulfils its purpose excellently well. It would have been better to have called it "An Introduction to the Study of the British Grasses," as it only deals in detail with the British species, which are not more than one-thirtieth of the total number of grasses that are known in the whole world. The short introduction explains how easily a collection of dried grasses can be made. The first chapter, called "Structure," gives all the different organs in detail, showing what is the general plan on which grasses are organised, and explaining the general and special terms which are used in describing the genera and species. In the second chapter, which is the longest in the book, the hundred and odd British species are classified according to their localities, and described in detail, most of the common kinds being illustrated by small woodcuts, with dissections. The third chapter is devoted to classification, in which Bentham and Hooker's "Genera Plantarum" is followed. The British genera are described in detail, and the characters of the thirteen tribes there adopted, several of which are not represented in Britain, are given. The rest of the book is occupied by a readable account of the geographical distribution of the grasses, especially of the cereals, and an account of their various uses for food, and in other ways. *Gramineæ* is one of the most universally distributed of all the natural orders of plants, and, in point of the number of species, is only exceeded by five other natural orders: *Compositæ*, *Leguminosæ*, *Orchideæ*, *Melastomaceæ*, and *Rubiaceæ*. Between three and four thousand species of grasses are known, and they are classified under three hundred genera. The little book is well written and trustworthy, and no doubt will secure a good circulation.

Rural Water Supply. By Allan Greenwell, A.M.I.C.E., and W. T. Curry, A.M.I.C.E. Pp. 210. (London: Crosby Lockwood and Son, 1895.)

IN this volume we have an elementary work on water engineering, containing a sufficient account of the principles and construction of waterworks to be of real use to engineers, and forming at the same time a good introduction to more elaborate treatises. The volume is based upon a series of articles which appeared in the *Builder* last year, and it contains valuable information upon all matters connected with water supply. It is, indeed, what its secondary title represents it to be, namely, "a practical handbook on the supply of water and construction of waterworks for small country districts." The book is full of details on points which are continually before waterworks engineers; and though these details are mostly rules and formulæ which have to be accepted without being understood, they will be of great assistance in planning schemes of water supply and in carrying out the works.

Climbing in the British Isles. II. Wales and Ireland. Wales. By W. P. Haskett Smith. *Ireland.* By H. C. Hart. Pp. 197. (London: Longmans, Green, and Co., 1895.)

CLIMBERS will find this little pocket-book an invaluable guide to instructive scrambles in Wales and Ireland; but the large number of fatal accidents recorded in its pages is hardly calculated to give other readers the mountaineering fever. On the first two pages of the book, three fatal falls and one severe accident are noted, and the tale of deaths is sustained throughout the book. To those who are filled with the desire to climb, this spice of danger only gives zest to the recreation; and the fact that several lives have been lost in attempts to scale a certain rock, is a sufficient reason for many Englishmen to tackle that rock and endeavour to scale it. In the book under notice, all the essential information about climbs in Wales and Ireland is given, with thirty-one illustrations (by Mr. Ellis Carr) and nine plans. By means of it, the would-be climber will be able to select his hills and peaks without difficulty, and with its assistance he may do in these islands hill-climbing which will form no mean part of a real mountaineering education. The book is primarily intended for those who climb for climbing's sake, hence little attention is paid to the geological interest of the rocks and hills described.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Feeding-Ground of the Herring.

IN his presidential address to Section D of the British Association at Ipswich, Prof. Herdman says:—

"Probably no group of animals in the sea is of so much importance from the point of view of food as the Copepoda. They form a great part of the food of whales, and of herrings and many other useful fish, both in the adult and in the larval state, as well as of innumerable other animals, large and small. Consequently, I have inquired somewhat carefully into their distribution in the sea, with the assistance of Prof. Brady, Mr. Scott, and Mr. Thompson. These experienced collectors all agree that Copepoda are most abundant, both as to species and individuals, close round the shore, amongst seaweeds, or in shallow water in the Laminarian zone over a weedy bottom. Individuals are sometimes extremely abundant on the surface of the sea amongst the plankton, or in shore pools near high water, where, amongst *Enteromorpha*, the Harpacticidæ swarm in immense profusion; but, for a gathering rich in individuals, species, and genera, the experienced collector goes to the shallow waters of the Laminarian zone. . . . In order to come to as correct a conclusion as possible on the matter, I have consulted several other naturalists in regard to the smaller groups of more or less free-swimming Crustacea, such as Copepoda and Ostracoda, which I thought might possibly be in considerable numbers over the mud. I have asked three well-known specialists on such Crustaceans—viz., Prof. G. S. Brady, F.R.S., Mr. Thomas Scott, F.L.S., and Mr. I. C. Thompson, F.L.S.—and they all agree in stating that, although interesting and peculiar, the Copepoda and Ostracoda from the deep mud are not abundant either in species or in individuals. In answer to the question which of the three regions, (1) the littoral zone, (2) from low water to 20 fathoms, and (3) from 20 fathoms onwards, is richest in small free-swimming, but bottom-haunting, Crustacea, they all replied the middle region from 0 to 20 fathoms, which is the Laminarian zone and the upper edge of the Coralline. . . . [Mr. T. Scott] tells me that as the result of his experience in Loch Fyne, where a great part of the loch is deep, the richest fauna is always where banks occur, coming up to about 20 fathoms, and having the bottom formed of sand, gravel, and shells. The fauna on and over such banks, which are in the Coralline zone, is much richer than on the deeper mud around them. On an ordinary shelving shore on the west coast of Scotland, Mr. Scott, who has had great experience in collecting, considers that the richest fauna is usually at about 20 fathoms."

It seems to me that these three specialists, or experienced collectors, have not given Prof. Herdman any information as to whether free-swimming Crustacea, such as Copepoda, are found in considerable numbers over the mud or not, as maintained by Dr. Murray in his concluding remarks in the Summary volumes of the *Challenger* Report, and I propose to answer the

question here. For ten years I have been engaged in dredging and trawling about the coasts of Scotland, chiefly as captain of Dr. Murray's yacht *Medusa*, and my experience does not coincide with that of Messrs. Brady, Scott, Thompson, and Herdman. For instance, in Loch Fyne I have always been able at any time of the year to collect in half an hour enormous numbers of *Euchæta*, *Calanus*, and *Nyctiphanes* over the mud in depths of about 70 fathoms or greater. Any person can see at the Millport Biological Station large bottles filled with these Crustaceans taken in a single haul. The stomachs of the herrings are frequently crammed with these Crustaceans, and the herrings certainly never got this food about the Laminarian zone, as suggested in Prof. Herdman's address.

Dr. David Robertson, who is one of the best-known collectors in the country, pointed out years ago that the Loch Fyne herrings got their food in the deep water, and attributed their fine quality to this fact. Dr. Robertson authorises me to say that, though there may be more species of Copepods in the Laminarian zone than in the deep water, still the number of individuals is very much greater in the deep water over the mud, as is conclusively proved by the *Medusa's* work.

Proper methods must, of course, be used, for I know of at least one instance in which a gentleman of considerable scientific repute was prepared to say that the free-swimming Crustaceans over the mud had completely left Loch Fyne; he communicated his opinion to Dr. Murray, with the result that the *Medusa* was ordered to Loch Fyne to investigate the matter. As was expected, the result was that these Crustaceans were found in as great profusion as on any previous occasion.

The result of my experience in Loch Fyne is that the nearer the nets are dragged to the mud in the deep water the greater will be the number of *Euchæta*, *Calanus*, and *Nyctiphanes* captured. I have taken, hundreds of times, in 70 fathoms, in a single haul lasting from twenty minutes to half an hour, more Copepods than can be collected in the Laminarian zone in eight or ten days. I have also captured herrings by means of drift nets sunk to the bottom, in depths of 70 and 80 fathoms, and their stomachs were filled with Crustaceans of the same species as we captured by the nets just over the mud at these depths.

As to the deep mud in Loch Fyne not producing a rich fauna, I may state that in the deepest water the trawl could not be kept down for a longer time than about half an hour; otherwise the deck engine and all other appliances on board would have failed to bring the net to the surface through sheer weight, chiefly of organisms. There was generally a certain percentage of mud present, but the bag of the net was generally crammed with thousands of *Actiniæ*, which live there, along with *Pecten septemradiatus*, *Hippolyte*, *Pandalus*, *Crangon*, *Ascidians*, and many other invertebrates and fishes.

I have dredged Loch Fyne systematically for months, and examined its fauna from the littoral zone to the greatest depth; the specimens collected are now beside me, and all the journals with the notes are in Dr. Murray's possession. But I think enough has been said to show that the greatest abundance of Copepods is not to be found in the Laminarian or other shallow zones, but in the deep water over the mud; also that the deep mud does possess a very rich fauna. I speak only of the abundance of the above-mentioned organisms, with which I am well acquainted. I am not a specialist nor a scientific man, but I have had a great deal to do with the practical part of the investigations which have assisted Dr. Murray in drawing his conclusions. ALEXANDER TURBYNE.

Millport, Cumbrae, N.B., October 5.

The Toronto Meeting of the British Association.

AN effort will be made to have the meeting of the American Association for the Advancement of Science held at San Francisco in 1897, so that the members of the British Association may cross the continent, and join us there, either before or after their own meeting at Toronto, which many of us hope to attend.

A suggestion of great importance, and deserving immediate consideration, seems to me that the Australasian Association should try to arrange a meeting for the same year on the Pacific coast of America, so that we may all join in the meeting of the American Association at San Francisco. This will be the first meeting of any of these Associations on that coast, and hence a momentous occasion.

I do not know how to reach the officers of the Australasian Association; but think that an insertion of this letter in NATURE will find them. I have already sent a letter at a venture to the President by his official title, as I do not know his name, in care of the Post-master of Melbourne, to be forwarded; but perhaps the Post-master may not know where to send it.

I have also written to Mayor Sutro of San Francisco, calling his attention to it. WM. H. HALE.

Brooklyn, October 9.

The Theory of Magnetic Action upon Light.

IN the British Association Reports for 1893, Mr. Larmor has attempted to show that a satisfactory theory of magnetic action upon light can be constructed by means of a modification of Maxwell's theory which was proposed by Prof. Fitzgerald in 1879; and he alleges, with special emphasis (see p. 349), that his theory furnishes "a consistent scheme of equations of reflection and refraction, without the necessity of condoning any dynamical difficulties in the process." And on p. 359, after raising objections against a theory originally suggested by Prof. Rowland, and afterwards fully developed by myself, he says:—"But against this procedure," that is my own, "there stands the pure assumption as regards discontinuity of electric force at an interface."

To fully discuss the defects of Larmor's resuscitation of Fitzgerald's theory would occupy too much space, and would necessitate the introduction of a considerable amount of mathematical analysis. I shall, therefore, confine myself to pointing out that his theory is open to exactly the same objections as my own, viz. *discontinuity of the tangential component of electromotive force at an interface.*

One of Larmor's boundary conditions (see p. 349) is equivalent to the condition that the expression

$$\frac{4\pi g}{K} + 4\pi\frac{Cd\beta}{d\theta} - 16^2\pi C\gamma_0\frac{df}{dt}$$

should be continuous. Now $4\pi g/K = Q$, where Q is one of the tangential components of the E.M.F. at an interface; also in unmagnetised media $C = 0$. Consequently, if accented letters refer to the latter medium, the condition becomes

$$Q + 4\pi Cd\beta/d\theta - 16\pi^2 C\gamma_0 df/dt = Q';$$

in other words, *the tangential component of the E.M.F. is discontinuous.* A. B. BASSET.

Holyport, Berks, October 9.

The Society of Chemical Industry and Abstracts.

AT the recent annual meeting of the Society of Chemical Industry, the retiring President and the new President each made some remarks concerning the cost of the journal of the Society, and the necessity of curtailing expenses by dealing more strictly with the abstracts. I suppose hardly any two of us would quite agree as to what is the rubbish, Teutonic or otherwise, which ought to be left out, and what is good matter, which ought to be abstracted at greater or less length. No matter who is editor, all of us would abide as firmly as ever in the belief that we could have made a better selection of articles for abstraction. Before, however, we set about any further movement in the direction of cutting down abstracts to a mere useless list of titles, I would like to point out one direction in which expense might safely be curtailed without fear of objection from any quarter. All will agree, I am sure, that it is a waste of money to abstract the same article twice. I am sure other members besides myself must have noticed that this blemish is not entirely absent from the Society's journal. It should be known to every chemical babe and suckling, that even very unimportant papers are sometimes published more than once. Yet this seems to have escaped the notice of whoever is responsible for the editing of the abstracts. Witness the following from this year's journal:—P. 389, "Sulphides of Cobalt and Nickel, A. Villiers (*Bull. Soc. Chim.*, 1895, 13 [4])," and "*Qualitative* Separation of Nickel from Cobalt, A. Villiers, *Bull. Soc. Chim.*, 1895, 13 [4]." Now let us turn to p. 524, where we find, "Sulphides of Nickel and Cobalt, A. Villiers, *Comptes rend.*, 1894, 119," and on p. 509, "Qualitative Separation of Nickel and Cobalt, A. Villiers, *Comptes rend.*, 1895, 120." We have cobalt and nickel in one case, and nickel and cobalt in the other; but the articles from the *Bull. Soc. Chim.* are the same as those from the *Comptes rend.*, and by the same author. A still more incomprehensible example will

be found on comparing pp. 191 and 313. On p. 191 we have a short abstract of an article on petroleum, by A. Riche and G. Halphen. On p. 313 we have a long abstract of the same article. In one case it is given under *qualitative* organic chemistry, in the other under *quantitative* organic chemistry. Yet the reference in each case is the same—"*J. Pharm. Chim.*, 1894, 30, 289." In this case, therefore, the abstracts are not even prepared from different journals.

I would suggest, then, that the first reform which the Editing Committee might institute in carrying out their scheme of retrenchment, should be one placing a limit on the number of abstractors who are to deal with one and the same article, even when it occurs in different publications. JAMES HENDRICK.

Glasgow, October 2.

Note on the Dendrocolaptine Species, "Dendrexetastes capitoides" of Eyton.

IT recently became necessary for me to examine some of the Dendrocolaptine birds in this museum, and among them the species named above. Our specimen, the type of the genus *Dendrexetastes* founded by Eyton in 1851, in Jardine's "Contributions to Ornithology," on a skin from an unknown locality, formerly in Lord Derby's museum, has evidently been examined by Dr. Sclater, for its label bears, in the well-known calligraphics of that distinguished authority on this group, the name *Dendrexetastes temmincki*. The difficulty I have in ascribing our specimen to that species is the cause of this note. According to the fifteenth volume of the "British Museum Catalogue of Birds," by Dr. P. L. Sclater, the genus contains but two species, *D. temmincki* and *D. devillii*, which, by his key on p. 140, are distinguished from each other, the former by having "blackish cross-bands" on the belly, and the latter having that region "uniform brown." On consulting Eyton's original description in the "Contributions to Ornithology," I can find no mention of any cross-bands on the belly; for there are none on the skin, which is apparently that of a mature bird. In looking up next the description by Lafresnaye, in the "Revue de Zoologie" for 1851, of his *D. temminckii*, to which Dr. Sclater has relegated as a synonym Eyton's *D. capitoides*, I read:—" . . . . pectoris ventrisque plumis totis umbrinis, in medio maculâ triangulari-elongata nivea nigro marginata notatis; ventris maculis strictis; fere linearibus; subcaudalibus pallide rufescentibus, albo late, fuscoque anguste vittatis. . . ." These words, as I interpret them, make no mention of the presence of cross-bands on the belly of *D. temmincki*, while the latter half of the quotation, in regard to the under-tail-coverts being pale rufous, with broad white and narrow fuscous spots, does not apply to *D. capitoides*, for the type-skin before me presents no such characters. The plate illustrating Lafresnaye's description of the first-mentioned bird (*loc. sup. cit.*) shows its breast-spots to be much narrower, though not linear, and shorter than those in *D. capitoides*, while the spots on the feathers on the upper part of the belly can hardly be termed "fere linearibus," which they are, however, in *D. capitoides*. The lower belly in the plate, "plumis totis umbrinis," shows, just as in the last-mentioned species, not a single cross-band. It would appear to me, therefore, that *D. capitoides*, Eyton, can scarcely be = *D. temmincki*, Lafr., while the latter differs from *D. devillii* (of which I regret our museum does not possess a specimen), and, I take it, from *D. capitoides*, by its smaller and narrower throat-spots. The subcaudal characters separate *D. capitoides* from *D. temmincki*, and apparently the typical *D. devillii* is separated from it also by the "striis strictissimis" of the breast, and the very linear shaft-stripes of the upper neck feathers. Is *D. capitoides* = *D. devillii?* Or are there three species? I incline to the opinion that there are. HENRY O. FORBES.

The Museums, Liverpool, October 8.

The Pressure of a Saturated Vapour as an Explicit Function of the Temperature.

IT may be of some interest to note that Cailletet and Mathias' "Law of Diameters," in combination with any equation of state, such as Van der Waals', which applies to the region of coexistence of liquid and vapour, supplies an (empirical) expression for the maximum pressure of a vapour at any temperature T in the form of an explicit function of this temperature and known constants.

Let p, v and T denote the pressure volume and absolute temperature of unit mass of the substance. According to Van der Waals' original equation of state, we have then :—

$$\left(p + \frac{a}{v^2}\right)(v - b) = \mathrm{RT}.$$

If v_1, v_2, v_3 denote the roots of this cubic in v, we have :—

$$v_1 + v_2 + v_3 = b + \frac{\mathrm{RT}}{p} \quad \ldots \ldots \text{(i.)}$$

$$v_1 v_2 + v_2 v_3 + v_3 v_1 = \frac{a}{p} \quad \ldots \ldots \text{(ii.)}$$

$$v_1 v_2 v_3 = \frac{ab}{p} \quad \ldots \ldots \text{(iii.)}$$

Now, for any definite value of T less than the critical temperature, these equations give, when we put p equal to the maximum vapour-pressure corresponding to this temperature, three values, v_1, v_2, v_3, two of which (say v_1 and v_2) denote the volumes of unit mass of the substance in the states of saturated vapour and "saturated" liquid at this temperature. Accordingly, $\frac{1}{v_1}$ and $\frac{1}{v_2}$ denote the densities of the substance in these states, and the law of Cailletet and Mathias, above referred to, enunciates that the arithmetic mean of these densities can be very fairly represented as a linear function of the temperature. Therefore we can write :—

$$\frac{v_1 + v_2}{v_1 v_2} = \phi \mathrm{T} \quad \ldots \ldots \text{(iv.)}$$

where ϕ denotes a linear function, whose two constants are known.

Eliminating v_1, v_2, and v_3 from the four equations (i.), (ii.), (iii.) and (iv.), we readily obtain :—

$$p = \frac{\mathrm{RT}(1 - b\phi\mathrm{T}) - a\phi\mathrm{T}(1 - b\phi\mathrm{T})^2}{b^2\phi\mathrm{T}} \quad \ldots \text{(v.)}$$

This result simply amounts to the following :—

If we fix the temperature T of a vapour, then the maximum vapour-pressure at this temperature is completely determined, *i.e.*

$$p = \mathrm{F(T)}.$$

Similarly the sum of the densities of saturated vapour and liquid in contact with it is determinate if T is fixed, and thus

$$\frac{1}{v_1} + \frac{1}{v_2} = \phi(\mathrm{T}).$$

Equation (v.) shows that the former function is known if the latter be known, and as Cailletet and Mathias have shown that the latter is very approximately linear, we can give the form of F(T).

This result, however, is not of any practical use unless the equation of state does really apply with good approximation to the region of liquid and vapour. F. G. DONNAN.

Holywood, Co. Down.

Colours of Mother-of-Pearl.

IN numerous text-books the colours of mother-of-pearl are included amongst phenomena of colour produced by striated surfaces, and though it is conceded that only a part of the colour is due to this cause, that part is generally assumed to be, at any rate, an appreciable quantity. Experiment will show, however, that such is not the case. When the colour produced by the striations is viewed in an impression of the pearl on sealing-wax or gelatine it is visible, though it is totally different in character from the iridescence of the pearl itself, in which the tiny contribution of colour from the striations is completely overpowered by that due to another cause. In white mother-of-pearl the striations are often as close together as in coloured varieties, and at certain angles, when viewed by light from a defined source, there is a little colour visible in the white specimens; just so much, and no more, is contributed by the striations of the coloured specimens, as may be shown by viewing a piece under the surface of water, when the effect of the striations is necessarily abolished, though the iridescence is not at all appreciably diminished. The whiteness of some varieties must be attributed to a different thickness or greater opacity of the laminæ. It is

these laminæ which, acting as "films," give rise to all the colour of nacre, practically; and the phenomenon should be included amongst those of colours from "films," and not from "striated surfaces," the latter being merely incidental, and for all practical purposes contributing nothing to the effect.

C. E. BENHAM.

A RATIONAL CURE FOR SNAKE-BITE.

WHEN it was established beyond dispute or cavil that the serum obtained from animals, immunised against bacterial infections and intoxications, possesses in a marked degree antitoxic powers—as distinguished from antibiotic powers—and that such serum when mixed in a test-tube with the bacterial poison in question will, so to speak, neutralise the toxic effects of such poison, however deadly, it was merely a question of time, opportunity, and patience that attempts would be made to extend the principle of serum-immunisation to other, *i.e.* non-bacterial, poisons. Ehrlich was the first to show us the way. He gradually accustomed animals to withstand comparatively large doses of abrine, ricine, and robine, three vegetable toxines, all belonging to the group of proteines, reacting as albumoses or globulines. In that manner he produced in the animals a relative immunity, or perhaps, more correctly, a tolerance. He found that though subcutaneous inoculations lead to better results, that this immunity can be brought about also by feeding. In whatever way the animal is prepared, its serum eventually acquires specific antitoxic, immunising, and curative properties. It was thus demonstrated that the wonderful discovery of Behring and Kitasato—for which Behring, however, claims the sole credit—has a scope much wider than at first was dreamt of. Behring himself, to begin with, explained the action of the serum as antibiotic or germicidal; but it soon became evident that, though when injected into the animal body it causes the destruction and death of the infective pathogenic organisms, nevertheless its chief action is "vitally" antitoxic. For working with the tetanus toxine alone, separated from the bacilli which produced it, its deadly effects can be readily neutralised by a few cubic centimetres of a powerful serum. And if we remember that ·23 milligram of tetano-toxine would represent the fatal dose for a human being weighing 70 kilogrammes, then we can get an idea as to what extraordinary changes must have been produced in the serum, or rather in the blood and tissues, of the immunised animal, to enable its serum instantaneously to remove the lethal effect of the toxine. The only poison comparable to tetano-toxine in virulence and rapidity of action is cobra poison, and it also resembles chemically the bacterial toxines, reacting as an albumose, though for the sake of accuracy it must be mentioned, that the poison of tetanus has been clearly shown by Brieger, Cohn, and Sidney Martin not to be an albuminous body, and that possibly most of the bacterial toxines may turn out not to be albuminous substances. Still, so far as our present knowledge reaches, cobra poison and other snake venoms are chemically closely allied and analogous to the "toxalbumins" of bacteria.

It had also been demonstrated by several observers,[1] that by means of oft-repeated injections of small sub-lethal doses of snake poison (rattlesnake, cobra, or viper venom) the resistance of an animal against the poison may gradually be increased considerably, it may be rendered "giftfest," to borrow a German expression. In fact, all the methods used for inducing a tolerance against tetanus poison can be shown to work in the case of cobra poison (this is the poison generally employed). Thus Calmette, whose work in this line follows directly that of Sewall's and of the writer of this article, has shown that a so-called immunity can also be produced by gradually increasing injections of poison attenuated by heat, iodine, trichloride of iodine, hypochloride of calcium, &c.; in fact, the analogy is complete. From this stage, at which others had already arrived, Calmette went ahead with Phisalix and Bertrand. Having previously attempted both to prevent and to cure the effects of inoculation with cobra poison by means of chloride of gold—wherein, however, as shown by the writer,[1] he failed—he directed his attention at once to the serum of immunised animals, and in February 1894 he showed, before the Société de Biologie, that on mixing cobra or viper venom with small quantities of serum obtained from an immunised rabbit the deadly effect of the venom disappears, a fact at once confirmed by independent observations of Phisalix and Bertrand. In May 1894 and in April 1895, Calmette published two concise papers in Pasteur's *Annales*, containing a full account of his results. These, briefly summarised, are as follows: (1) The serum of an animal immunised against snake poison (he used poisons of the following snakes: *Naja tripudians and haje*, *Crotalus durissus*, *Bothrops lanceolatus*, *Cerastes*, *Pseudechis porphyriacus*, *Hoplocephalus curtis and variegatus*, *Acanthopis antarctica*, *Trimeresurus viridis*) possesses properties similar to those which the serum of animals immunised against tetanus and diphtheria possesses. (2) The serum of a rabbit immunised against cobra or viper venom acts equally well against any of the other poisons, *i.e.* there is no specificity of action, as judged by the species of snake. (3) The serum possesses not only neutralising properties when mixed with the venom in a test-tube, but possesses also marked immunising and curative properties, *i.e.* poison injected after previous serum administration becomes powerless, and serum injected after previous poison administration neutralises the effects of the poison in the animal body, even after the symptoms of intoxication have already set in. Naturally the effect depends on the degree of immunity of the serum giver and on the proportionate amount of serum used. (4) The immunising effect produced by serum injections is not so lasting as that produced by direct injections of the poison, *i.e.* serum injections are incapable of rendering animals "giftfest." Calmette alludes to other matters, but since these are of secondary importance and still debatable, and not directly related to the subject of this article, we must pass them over. There is, however, one point which must be mentioned, since it is one affecting the whole principle of serum immunisation. He states that he has succeeded in producing a "Giftfestigkeit" by means of repeated intravenous injections of hypochloride of calcium, and that the serum of such "chlorinated" animals will neutralise, in the test-tube at least, the effects of cobra poison. Roux elsewhere mentions[2] that the serum of animals immunised against tetanus or rabies is capable of neutralising snake venom and of protecting other animals against subsequent intoxication with cobra poison, and that rabbits vaccinated against rabies can withstand four to five times the lethal dose of cobra venom; and also that abrine serum will counteract the effects of cobra poison, and cobra serum those of abrine. Calmette goes so far as to say that an animal vaccinated against abrine may acquire a relative immunity against diphtheria, ricine, and anthrax. If this be so, we shall have to modify our views as to the specific action of antitoxic serum, *i.e.* the first principle of serum therapeutics. We require a number of control observations before we can accept these remarkable statements; partial contradiction they have already received from Germany,[3] and the

[1] Sewall, *Journal of Physiology*; Kanthack, *ibid.*, 1892, vol. xvi. Nos. 3 and 4; Phisalix et Bertrand, *Compt. rend. de l'Acad. d. sc.* cxviii. 1894, pp. 288, 356; *Arch. de physiol.* 5 sér. vi. 3, 1894, pp. 567, 611; *Compt. rend. de la soc. d. biol.* 9 sér. vi., 1894, pp. 111, 124; Kaufmann, *ibid.*, p. 113, and Calmette, *Annales de l'Inst. Past.* 1894, vol. viii. No. 5, p. 281.

[1] *Lancet*, June 11, 1892. The uselessness of strychnine was previously demonstrated by the writer in his paper in the *Journal of Physiology*.

[2] *Annales de l'Inst. Past.* 1894, No. 10, p. 722.

[3] Ehrlich emphatically denies any such vicarious counteraction with regard to abrine and ricine (cf. *Deutsche Med. Wochenschrift*, vol. xvii. No. 44, p. 1218).

writer's own experiments, so far at least, do not lend much support to them. So long, however, as the whole question of this new treatment, striking though it is in its results, is still a mystery to us, we cannot afford to push aside observations because they seem improbable, or because they are contradictory.

Calmette asserts also that the fresh serum of *Naja tripudians* (a species of cobra) possesses to some degree at least immunising properties, and, as we shall see, Fraser[1] bears him out in this, by stating that fresh serum of poisonous snakes possesses strong antitoxic and protective properties, not only against their own venom, but also against that of other species. D. D. Cunningham[2] and the writer,[3] however, in India, invariably failed to obtain antitoxic or immunising effects with cobra blood or serum, although the writer succeeded in keeping the effects of cobra poison in abeyance by means of the blood (or serum) of the *Varanus Bengalensis*, a large lizard which is naturally strongly resistant against cobra poison.

These are the chief results obtained by Calmette, and knowing the difficulties of working with such deadly poison as cobra poison venom is, and the innumerable failures which accompany it, the writer is able to appreciate the success of the French author, all the more since he himself failed while working on the same lines where to succeed seemed simply a matter of course. Recently these French observations have received entire confirmation in their leading points by Prof. Fraser of Edinburgh, and the writer may be forgiven for stating here that though he took up the control of Calmette's work with strong bias against the latter, he felt himself forced, already before Fraser's communications appeared, to acknowledge the correctness of the work done at Pasteur's Institute, so far as the antitoxic and immunising properties against cobra poison of serum obtained from animals treated with that poison are concerned. He has not, however, convinced himself that hypochloride of calcium can immunise animals, or lead to the formation of an antitoxic serum. Fraser's contributions, though merely confirmatory, are of great importance, since they contain unquestionable proof of the truth of what must have appeared to all, except a few shrieking "zoophilists," to be striking and surprising revelations. The credit, however, of the discovery of a cure for snake-bite—in the laboratory at least—belongs solely to France. Having discussed Calmette's work more fully, we can speak of Fraser's experiments in a few words; but thereby we do not wish to detract in any way from the merit which characterises his researches.

Fraser[4] worked with venom obtained from the Indian cobra, three species of rattlesnakes (*Crotalus horridus*, *C. adamanteus*, and *C. durissus*), the copper-head (*Trigonocephalus contortrix*), the Australian black and brown snakes, and an unidentified *Diemenia* (*Pseudechis porphyriacus* and *Diemenia superciliosa*), the African puff-adder, night adder, yellow cobra, and "rinkas" (*Vipera arietans*, *Aspidelaps lubricus*, *Naja haje*, *Sepedon hæmachates*). He immunised his animals by the usual method of minimal subcutaneous inoculations, or by feeding, against the venoms of some of the snakes mentioned, and then established (*a*) the strong specific antidotal properties of the serum of these vaccinated animals against the poison with which they had been vaccinated, and (*b*) the vicarious antidotal properties against the other poisons. This serum he obtained in a dry, pulverisable condition without any appreciable loss of antidotal power; but we can hardly forgive him the hybrid and barbaric name "antivenene" which he applies to it. He confirms Calmette's results in almost every point, so that there is no longer any doubt left as to possibility of a successful cure against snakebite, especially as, by both observers, the curative injection was shown to be efficacious when the symptoms of intoxication had already set in, and as the experimental animals used were highly susceptible to the poisonous action of serpents' venoms, while man is weight for weight much less sensitive than a guinea-pig or a rabbit. True, Fraser has generally worked with comparatively small lethal doses; this possible objection is, however, met by Calmette's results, which were obtained with much larger doses, and which therefore allow us to judge favourably of the practical application of the serum treatment. The final verdict must, of course, depend on the success or failure following the use of the serum in cases of snake-bite, and it must be remembered that, striking though our laboratory results are with tetanus-antitoxine, so far the success obtained with acute cases of tetanus in man is disappointingly small, as the writer has shown elsewhere.[1] Yet here we have a rational method of treatment, and the promise of almost certain success; we must now look for facilities and opportunities of trying the cure. In France they have already begun to manufacture this antitoxic serum in larger quantity, and Calmette writes that he has immunised a horse, and is ready to supply the remedy; and Fraser also has larger animals under treatment. No doubt India will not delay in carrying out the necessary arrangements for procuring what, after all, will be an imperial benefit.

The vicarious action of the immunising venom-serum is surprising, and may find an explanation in the similarity of the physiological action of the various poisons used. They are all poisons which cause death by acting on the central nervous system, especially the medulla, the animal dying from respiratory failure with salivation, retching, &c. And it is quite possible that chemically similar poisons which, according to their action on the animal body, belong to one physiological group, have the same antidote. It would therefore be interesting to test the antitoxic cobra-serum on the poison of the Daboia, which, according to Wall, Cunningham, and others, differs essentially in its physiological action; for whereas cobra, crotalus, and viper venoms are paralysing, medullary poisons, the poison of Russell's viper produces very varying symptoms, in some cases convulsions, in others paralysis and asphyxia, in yet others violent convulsions followed by paralysis. Daboia venom undoubtedly contains a substance capable of producing the most violent convulsions, especially in birds, their occurrence depending on the size of the animal and on the amount of poison injected. It would indeed be more than a surprising revelation, if a serum which is capable of acting as an antidote to a paralysing toxine were also capable of neutralising the effects of a toxine of opposite physiological action.

The vicarious antidotal action of venom-serum must appear all the stranger and more contradictory if we remember that not all poisonous snakes are "giftfest" against the poisons of other different species. Waddell[2] has shown that the venom is neither a poison to the snake itself nor to members of its own species, but that cobra poison is fatal to some, if not perhaps to all, poisonous snakes. It will certainly kill the *Trimeresurus erythrurus*, and in the writer's experience also the crotalus, while according to Fayrer the *Bungarus* readily falls a victim to the bite of a cobra. This being so, why should the antitoxic serum of an animal immunised against cobra-poison be active against rattlesnake venom, when in an experiment recently performed by the writer, a strong and healthy crotalus succumbed to five milligrammes of cobra venom? Lastly some writers, Fraser included, assume that the immunity of poisonous snakes against their own

1 *Lancet*, August 10, 1895, p. 376, and *Brit. Med. Journal*, Aug. 17, 1895.
2 Private communication.
3 *Journal of Physiology*, 1892, vol. xiii. Nos. 3 and 4, p. 288.
4 *British Medical Journal*, 1895, June 15, p. 1309-1312.

1 *Medical Chronicle*, May 1895.
2 "Scientific Memoirs by Medical Officers of the Army of India," 1889, iv. p. 59.

poison depends on self-immunisation, called forth by swallowing their own venom, or by repeatedly inoculating themselves. This is highly improbable, if we remember that some of the innocent snakes are very resistant against cobra poison, as, *e.g.*, the *Ptyas mucosus* and the *Tropidonotus natrix*, and also that, as the writer has shown, the *Varanus Bengalensis* is possessed of a marked tolerance, and that, according to Fayrer, other species of *Varanus* survive the bite of a cobra 24 to 48 hours. Jourdain further gives a list of four innocent snakes which are immune against viper venom. In what manner are we to account for this immunity? Interesting observations on the poisonous nature of serum of innocent and poisonous snakes are also found in Calmette's paper of April 1895, which, while rendering Fraser's theory still more improbable, do not assist us in clearing up the mystery. The explanation must be left to future researches; for the present we must be thankful for the promise which the researches of Calmette and Fraser have given us, of allaying an almost national calamity.

A. A. K.

SCIENTIFIC KNOWLEDGE OF THE ANCIENT CHINESE.

THE question of China has been so much to the front lately, that an article which appeared in one of the August numbers of the *Revue Scientifique*, on the knowledge of science possessed by the Chinese, seems very *à propos*. It cannot be denied that the Chinese of the present day have very elementary ideas on any branch of science. This however, was not so formerly.

In early times, as far back even as 2000 B.C., we find that science in China had reached a fairly advanced stage. The Chinese possessed undoubtedly a great knowledge of astronomy; inscriptions have been found which prove this. In the "Chou-King," a book of records, we read that Emperor Yao, who reigned 2357 B.C., did much to advance the study of this science. He ordered his astronomers to observe the movements of the sun, moon and stars, and showed them how to find out the commencement of the four seasons by means of certain stars. We read also that he told them that a year consisted of a little less than 366 days, and as he divided the year into lunar months, he taught them the years in which the additional lunar month ought to be included. It is also known that the Chinese had the annual calendar, that they observed the planets Mercury, Venus, Mars, Jupiter, Saturn, and were able to calculate eclipses, and knew the difference between the equator and the ecliptic. It is quite probable that the ecliptic was not known of before the Mussulmans occupied the Mathematical Tribunal, which they held for three centuries.

We see, therefore, that the knowledge of astronomy was very extensive. With regard to the meridian, it was apparently unknown to them. M. Chavannes, who is at present Professor of Chinese at the College of France, says that it is not mentioned in any astronomical book. As substitute a certain star was observed at the same hour, according to the times of the year, note being taken of its positions with regard to the horizon.

Astronomy has always been closely connected with astrology. By means of astronomy the time was ascertained for the numerous public ceremonies recorded in the Imperial calendar; it likewise regulated the affairs of the Government. But the calendar has long since ceased to be used for this latter purpose, and the majority of the Chinese population merely look upon it as a means of continuing the mysterious ceremonies and oracles connected with the different positions of the planets. It is ordered in the "Collection of the Laws," that at each eclipse, ceremonies should be gone through to deliver the eclipsed sun or moon. At this time therefore, an alarm is sounded on the drums, the mandarins arrive armed, utter many objurgations, and thus deliver the endangered bodies.

In the seventeenth century, certain Jesuit missionaries arrived in China. On seeing the low state into which the Mathematical Tribunal had fallen, they offered to help it. They found an observatory containing many instruments, which shows plainly that this branch of science had at one time reached an advanced stage. This decay of science is not to be wondered at when we remember that twenty-two dynasties were brought on the throne by actual revolutions. Nor is this decay confined to astronomy. According to the ancient books and traditions, we find that various branches of science had reached a high degree of culture.

The Emperor Kang-hi, who reigned in the seventeenth century, had a great love of study himself, and endeavoured to advance the general education in China. The Jesuit missionaries instructed him in geometry and physics. He translated some text-books into Chinese.

The Chinese have generally been credited with the invention of gunpowder. A certain document has been found, however, by Archimandrite Palladius, a Russian sinologue, stating that in the ninth century a Persian regiment, under the Chinese sovereign, made known a material similar to wild fire, which was afterwards used for fireworks.

Apparently, chemistry has never been studied, unless by a certain sect, the Tao-tse, who spent all their time endeavouring to discover the philosopher's stone and the elixir of life.

The Chinese have not a great knowledge of geology. The mines have been worked without any machinery, and are not very deep, therefore fire-damp has rarely been the cause of destruction. Coal was extracted at as early time as 200 B.C. in the dynasty of Han. Although the mode of extraction was very primitive, enough was obtained to satisfy all wants.

About 1861 the Government handed the exploration of the mines over to American prospectors. The work, lasting from 1862-64, was directed by Prof. Pumpelli, who at its termination sent the Emperor a report and a map of the coal-fields. The Smithsonian Institute of Washington have had these documents published; they have also appeared in the diplomatic correspondence of the United States (1864). Later on, Baron de Richtofen did similar work, and found that the coal-fields in China are even more extensive than those in North America.

Research work has not been carried far in natural science. In zoology their classifications are quite wrong. The drawings in zoological and botanical books can often scarcely be recognised. Their most ancient work on botany dates from 2700 B.C., and is a treatise written by the Emperor Shen-nung; it is merely enumerative. Another work, the "Rh-ya," dates from 1200 B.C., and shows signs of progress. The "Pen-tsao," an encyclopedia, is, according to M. Bretschneider, of little value.

This Russian investigator speaks of the Chinese as follows: "It is an undeniable fact that the Chinese do not know how to observe, and have no regard for truth; their style is negligent, full of ambiguities and contradictions teeming with marvellous and childish digressions."

However, in a more recent communication, M. Bretschneider retracts his words, and says that it is more that the Chinese will not observe, than that they cannot, for Lichi-Tchen, author of several interesting pamphlets, brings forward many facts concerning cultivated plants.

With regard to medical science, it is very elementary. Occasionally here and there a successful doctor is to be found. This lack of knowledge is not to be wondered at, for Buddhism forbids dissection of bodies. In the temple of Confucius a bronze figure is to be found, on which all the different parts are marked where the surgical needle

may be applied. This needle is practically the only instrument used in the profession.

The height of civilisation in China was reached at the end of the reign of Kang-hi. The gradual decline is supposed to have commenced with the Tartar domination.

THE FLORA OF THE GALAPAGOS ISLANDS.

DR. G. BAUR'S theory of the origin of the Galapagos Islands is too well known to need explanation here; yet it may be briefly designated the theory of subsidence. He argues that the islands were formerly connected with each other, and at an earlier period with the American continent. It is also almost needless to say that this theory has met with an exceedingly hostile reception; few indeed accepting it, even as restricted to a former union of the islands themselves. The publication of an account of the botanical collections[1] affords an opportunity of examining this theory from a botanical standpoint. For the purposes of the "Botany" of the *Challenger* Expedition, and ever since the publication of that work, I have collected all the data coming under my notice bearing on the dispersal of plants to considerable distances by wind, water, birds or other creatures excepting human. The evidence thus collected sufficiently accounts for the vegetation of low coral islands, and the littoral vegetation of widely separated countries; but it in no way helps to explain the vegetation of the enormously distant islands of the Antarctic seas, for example, or that of the islands of the Galapagos group, to give another instance.

But these are not parallel cases; they are the two extremes in the amount of differentiation in connection with isolation.

The biological phenomena of the Galapagos Islands left a deeper impression, probably, on the mind of Darwin than those of any other part of the world he visited, and doubtless had much to do with his later conception of the origin of species. The fact on which he laid special stress was that the genera, to a very great extent, were the same in all the islands, and the species different in each island. Dr. Baur's much more extensive zoological and botanical collections and observations confirm and emphasise the correctness of the view of his illustrious predecessor of fifty years ago. Darwin specially refers to the existence of different species or races of tortoises and mocking-thrushes in many of the islands; and Baur's examination of the lizards of the genus *Tropidurus*, from twelve of the islands, reveals the same condition of things. The botanists bring forward *Euphorbia viminea* in illustration of this phenomenon. This species was described by Sir Joseph Hooker from a single specimen collected by Macrae in Albemarle Island, and the author remarks that he "knew of no species with which to compare this highly curious one." Dr. Baur collected it extensively in eight of the islands, and the specimens from almost every one of them exhibit distinct racial characteristics. *Acalypha*, a genus of the same natural order, presents somewhat more pronounced variation in the different islands, which some botanists regard as of specific value; other botanists as of varietal value only. Whatever status we give these forms, the flora as a whole is a most instructive and convincing illustration of evolution.

A remarkable peculiarity of the Galapagos flora, as an insular flora, is the almost total absence of endemic genera, for the two or three genera of the Compositæ restricted to the islands are so closely allied to American genera as hardly to count as distinct. Indeed the whole flora is so thoroughly American that, apart from geological difficulties, it might be regarded as a differentiated remnant thereof, rather than derived therefrom, after the supposed elevation of the islands. Analogous conditions and phenomena are repeated in the deep valleys of the great mountain chains of northern India and western China, where, in neighbouring valleys, the genera are to a great extent the same and the species different.

Returning to Dr. Baur's extensive botanical collections from the Galapagos, it may be mentioned that they yielded about a dozen new species belonging to the predominating genera.

Looking at the composition of the Galapagos flora, especially with an eye to the probabilities of the transport of the seeds of its constituents, combined with present conditions, Dr. Baur's theory seems deserving of more serious consideration than it has hitherto received. My very slender knowledge of geology alone prevents me from taking up a more decided position.

W. BOTTING HEMSLEY.

[1] B. L. Robinson and J. M. Greenman, in *American Journal of Science*, vol. l. pp. 135-149.

N.B.—Dr. G. Baur was attached to the United States Fish Commission steamer *Albatross*, and spent nearly three months in the islands, from June 10 to September 6, 1891.

THE LATE PROFESSOR HOPPE-SEYLER.[1]

II.

Hoppe-Seyler's Work in Berlin, 1850-54 *and* 1856-61.

IT has already been stated that Hoppe selected as the subject of his inaugural dissertation some observations on the structure of cartilage and on chondrin.[2] Chondrin had been first separated and examined by Johannes Müller,[3] and afterwards by Mulder and Donders. Pursuing his study of the chemical reactions of the so-called chondrin, Hoppe in 1852[4] described its lævorotatory property, and showed that when decomposed by long boiling with dilute mineral acids it yields leucine, but neither glycocine nor tyrosine. Still directing his attention to the connective tissues, Hoppe in the following year published a valuable and interesting paper[5] on the structural elements of cartilage, bone, and tooth. Virchow had shown[6] the possibility of isolating the so-called bone corpuscles. Hoppe now alleged facts which seemed to prove that the lacunæ and canaliculi of bone are lined by a tissue resembling elastic tissue, and are left surrounding the bone cells when decalcified bone is boiled in a Papin's digester. Extending his investigation to tooth, Hoppe studied the chemistry of the organic basis of dentine, and isolated the "dentinal sheaths," which he showed to correspond structurally and chemically to the more internal portion of the ground substance of bone, which may be separated as a distinct investment bordering the lacunæ, canaliculi, and Haversian canals. There can be no question of the important bearing which these early histologic-chemical researches had upon the development of our knowledge of the relations and affinities of the connective tissues; attention has been drawn to them for this reason, as well as because they differed somewhat in their scope and method from the work with which Hoppe afterwards mainly busied himself.

Passing over three interesting papers on auscultation[7] and communications of minor importance on chemical

[1] In the fragmentary notes which follow, I do not pretend to give a complete or entirely consecutive account of Hoppe-Seyler's labours; my object is to draw attention to some of the principal results of his life-work, and to indicate in this way his position among those who, during the last half-century, have contributed to the advancement of biological science.—A. G.

[2] F. Hoppe, "De Cartilaginum Structura et Chondrino nonnulla," Diss. Inaug. Berol. 1850.

[3] Joh. Müller, *Poggendorff's Annalen*, vol. xxxviii. (1836) pp. 295-356.

[4] Hoppe "Ueber das Chondrin und einige seiner Zersitzungsproducte," *Journ. f. Prakt. Chemie*, vol. lvi. (1852) p. 129.

[5] Hoppe, "Ueber die Gewebselemente der Knorpel Knochen und Zähne," Virchow's *Archiv*, vol. v. (1853) p. 170.

[6] Virchow, "Verhandl. d. Phys. Med. Gesellschaft zu Würzburg," vol. ii. p. 152.

[7] Virchow's *Archiv*, vol. vi. (1854) pp. 143-173, vol. vi. (1854) pp. 331-349, vol. viii. (1855) pp. 250-259.

questions relating to physiology and pathology, we come to the first in the long series of valuable contributions which Hoppe made to the physiological chemistry of the blood. This short paper of only two pages was published in 1857, after his return to Berlin, and consisted of a preliminary communication on the action of carbonic oxide on the blood.[1] In this paper he announced that carbonic oxide so affects the colouring-matter (at that time designated Hämatoglobulin by Hoppe) as to render it incapable of fulfilling the function, so important for the blood as well as for the whole organism, of acting as the carrier of oxygen. Simultaneously and independently, Claude Bernard[2] had observed the same facts as Hoppe, and had shown, in addition, that when carbonic oxide acts upon blood it is absorbed and displaces oxygen. Although his analytical data did not bear out the assertion, Claude Bernard stated that for each volume of oxygen displaced one volume of carbonic oxide is absorbed, a relation which was afterwards shown to be actually correct by the fine investigation of Lothar Meyer.[3] As will be afterwards stated, it was, however, Hoppe-Seyler who, in 1865, after Stokes' beautiful researches on the reduction of oxy-hæmoglobin, furnished the complete explanation of the way in which carbonic oxide exerts its action on the blood and its colouring-matter, and placed in the hands of the medical jurist a method of distinguishing between blood which has been rendered florid by carbonic oxide and blood which owes its red arterial colour to oxygen.

The year 1857 witnessed also the publication of the first[4] of a series of researches on the property which many of the proximate principles of the body possess of rotating the plane of polarisation. Biot had discovered that albumin rotates the plane of polarisation to the left, and Bouchardat and A. Becquerel had endeavoured, but without success, to base upon this discovery a method for the quantitative estimation of albumin. In his first paper Hoppe showed (1) that, as was to be predicted, the rotation produced by a solution of albumin was strictly proportional to the amount of albumin in solution, and to the thickness of the stratum traversed by the light; (2) that albumin existing in a state of solution in a liquid rotates the plane of polarisation of light almost exactly as much to the left as an equal percentage of grape sugar rotates it to the right. In the same year (1857) and the year following, Hoppe published other papers on the rotatory properties of other organic proximate principles of the animal body.[5]

With his hands full of original work, with the chemical laboratory of the Pathological Institute to direct, busily helping the students who were attracted to work under a teacher full of enthusiasm and ability, Hoppe yet found time to publish, in 1858, the first edition of his "Handbook of Physiologico-Chemical and Pathologico-Chemical Analysis."[6] The only work at that time in existence which fulfilled the same object was the very useful work of Gorup-Besanez, of which the first edition appeared in 1850, the second in 1854,[7] and the third and last in 1871. Hoppe-Seyler's book was written on lines

[1] Hoppe, "Ueber die Einwirkung des Kohlenoxydgases auf das Hämatoglobulin. Vorläufige Mittheilung," Virchow's *Archiv*, vol. xi. (1857) p. 288.

[2] Claude Bernard, "Leçons sur les effets des substances toxiques et médicamenteuses." Paris, 1857, see p. 158.

[3] Lothar Meyer, "De sanguine oxydo carbonis infecto," Dissert. Inaugurat. Chymica. Vratislaviae, 1858.

[4] Hoppe, "Ueber die Bestimmung des Eiweissgehaltes im Urin, Blutserum, Transudaten mittelst des Ventke-Soleilschen Polarisations-Apparates," Virchow's *Archiv*, vol. xi. (1857) pp. 547-560.

[5] (*a*) "Ueber die Circumpolarisationsverhältnisse der Leim und Gallen Substanzen," Virchow's *Archiv*, vol. xii. (1857) p. 480; (*b*) "Bestimmung des Milchzuckergehaltes der Milch mittelst des Soleil-Ventke'schen Polarisationsapparates," Virchow's *Archiv*, vol. xiii. (1858) p. 276; (*c*) "Ueber die Circumpolarisirende Eigenschaft der Gallensubstanzen und ihre Zersetzungsproducte," Virchow's *Archiv*, vol. xv. (1858) pp. 126-141.

[6] I unfortunately have not by me this first edition, which I long treasured as my constant guide. If I remember rightly, the title of the first edition was "Anleitung zur Pathologisch-Chemischen Analyse."

[7] E. von Gorup-Besanez "Anleitung zur qualitativen und quantitativen Zoochemischen Analyse." (Nürnberg, Verlag v. L. Schrag, 1854.)

essentially the same, but was distinguished by containing many new methods, the results of the original researches of its author; as, for example, on the rotatory properties of various organic bodies, on the polarimetric estimation of albumin and milk-sugar, on the colorimetric estimation of the blood-colouring matter, on new methods of blood analysis, &c. Personally, the writer is greatly indebted to the first and the subsequent editions of Hoppe-Seyler's work, and in saying that it has exerted a powerful and useful influence in diffusing a knowledge of the best methods of practical work throughout the laboratories where researches in physiological chemistry are pursued, he is only expressing an opinion which he believes to be shared by all who are best qualified to judge. In spite of a decided narrowness, amounting at times to unfairness, which asserts itself in nearly all Hoppe-Seyler's writings, and which caused him to attach undue importance to his own work and that of his own pupils, and which explains some unfortunate omissions and deficiencies, the "Handbook" remains the recognised practical work consulted by the student of physiological chemistry. The sixth, and last, edition of the book,[1] edited jointly by Hoppe-Seyler and his pupil Dr. Thierfelder, appeared early in 1893.

Hoppe-Seyler's Work in Tübingen, 1861-72.

With his appointment as ordinary Professor of Applied Chemistry in the University of Tübingen commenced the most prolific period of Hoppe-Seyler's scientific life, during which he contributed to science his researches on hæmoglobin and its derivatives—researches which, with the work of Stokes, Claude Bernard, Pflüger, Ludwig and his school, have furnished us with the greater part of the knowledge which we at present possess concerning the chemistry of the blood-colouring matter and the part which it plays in respiration. At Tübingen, Hoppe, then in the very prime of life, surrounded by pupils, amongst whom were Diakonow, Dybkowsky, Miescher, Parke, and Salkowski, showed much more clearly than was possible in the position which he occupied in Berlin, his capacity to be the head of a school—that is, his power of inducing men to work out his own ideas, and of animating them with the desire to advance science by their own researches.

It was in 1862 that appeared Hoppe's short but epoch-marking paper "On the behaviour of the blood-colouring matter in the spectrum of sunlight."[2] Through the researches of Brewster and Herschel, the fact that absorption bands occurred in the spectrum of light which had been passed through certain coloured gases, vapours, and diluted coloured solutions had become known, and the absorption spectra of indigo and chlorophyll had been described. The discovery of the wonderfully characteristic absorption spectrum of blood at once enabled Hoppe to assert that hæmatin, which had up to that time been by many considered the true blood-colouring matter, did not exist preformed in the blood corpuscles, but that it is a product of decomposition of the true blood-colouring matter which is the cause of the absorption bands which he had discovered, and which, under the influence of heat, acids, &c., splits up into hæmatin and an albuminous substance. Without doubt, added Hoppe, the true blood-colouring matter is the body which forms the blood crystals of Funcke, and these crystals are not, as Lehmann had erroneously supposed, composed of a colourless albuminous *hæmatocrystalline* stained with hæmatin.

There can be no question that Hoppe at once appreciated the immense value of the information which

[1] "Handbuch der Physiologisch- und Pathologisch-Chemischen Analyse für Aerzte und Studirende," von Felix Hoppe-Seyler. Sechste Auflage neu bearbeitet von F. Hoppe-Seyler, Professor in Strassburg, und H. Thierfelder, Privat-docent in Berlin. (Berlin, Verlag von Aug. Hirschwald, 1893.)

[2] Prof. Felix Hoppe in Tübingen, "Ueber das Verhalten des Blutfarbstoffes im Spectrum des Sonnenlichtes," Virchow's *Archiv*, vol. xxiii. (1862), pp. 446-449.

he had acquired by his study of the spectrum of blood, though the full light which it was destined to throw on the function of the blood-colouring matter was only recognised when Stokes published his paper "On the Reduction and Oxidation of the Colouring-matter of the Blood." Having described the beautiful experiments which he had performed after becoming acquainted with Hoppe's paper on the blood spectrum, Stokes stated the conclusions, which might legitimately be drawn from them in the following words: "We may infer from the facts above mentioned that the colouring-matter of blood, like indigo, is capable of existing in two states of oxidation, distinguishable by a difference of colour and a fundamental difference in the action of the spectrum. It may be made to pass from the more to the less oxidised state by the action of suitable reducing agents, and recovers its oxygen by absorption from the air."[1]

The new facts acquired by the combined use of chemical and optical methods at once explained a large number of facts. Hoppe-Seyler showed that carbonic oxide blood was distinguished from normal blood in being unacted upon by reducing agents, and thus placed a valuable test in the hands of the medical jurist called upon to investigate cases of death by charcoal fumes.[2] The explanation of the facts discovered by Claude Bernard and by Lothar Meyer was obvious—to wit, that carbonic oxide forms a compound with the blood-colouring matter, more stable than the oxygen compound, and in which apparently one molecule of CO has replaced O_2.

With the resources of spectrum analysis to aid him, Hoppe now devoted himself with energy to the investigation of the blood-colouring matter (which he named Haemoglobin [3]), showing how to separate and purify it by repeated crystallisation, determining its composition, studying personally, and, with the aid of his pupil Dybkowsky, its combinations with oxygen and with carbonic oxide, examining its products of decomposition, and showing its probable connections with certain other animal colouring matters.[4]

It would be impossible in this place to comment in detail on all Hoppe-Seyler's contributions to the chemistry of the blood-colouring matter; these constitute his highest claim to distinction, and will ever cause him to be remembered as having contributed most largely to our knowledge of the manner in which the respiratory exchanges of animals are effected.

Until he removed from Berlin to Tübingen, and for some time after, Hoppe-Seyler published his researches for the most part in Virchow's *Archiv*, some of his papers appearing, however, in Fresenius' *Zeitschrift*, in the *Annalen d. Chemie und Pharmacie*, and in the *Berichte* of the Chemical Society of Berlin. In 1866, however, he commenced the publication of the collected papers issuing from his laboratory, under the title of "Med.-Chemische Untersuchungen."[5] Four parts of this publication appeared, the last in 1871.

Hoppe-Seyler's Work in Strasburg, 1872–1895.

A proper estimate of Hoppe-Seyler's work would necessitate a careful review of the fine researches published by his pupils, for there can be no doubt that in his case, as in that of many of the most distinguished scientific men of Germany, the work of the master has often been credited to the pupil under whose name it has appeared. It is obvious, however, that it would be impossible, within the limits of such an article as the present one, to give an account, however brief, of the succession of valuable papers which issued from the new Physiologico-Chemical Institute of Strasburg. Two events in Hoppe-Seyler's scientific life in Strasburg cannot, however, be passed over, viz. the publication of his "Text-Book of Physiological Chemistry," and the foundation of the *Zeitschrift für Physiologische Chemie*. The first part of the "Text-Book of Physiological Chemistry" appeared in 1877, the second in 1878, the third in 1879, and the fourth in 1881. This work is of interest as giving Hoppe-Seyler's views of the chemical processes of the body; yet it neither achieved nor merited great success. Devoted though he was to work by which he unquestionably did much to advance both physiology and pathology, Hoppe-Seyler was essentially a chemist rather than a biologist; and when, as in his systematic treatise, he left chemical, to speculate on biological, questions, his weak points became very obvious.

This account of Hoppe-Seyler's work must close with a reference to the great service which he rendered to our branch of science by founding, in 1877–78, the *Zeitschrift für Physiologische Chemie*. From the first number to the last this periodical has maintained a high standard, and, besides containing the results of all the work done in the Strasburg Laboratory, it has received contributions from nearly all the prominent workers in physiological chemistry. In succession to Hoppe-Seyler, Professors Baumann and Kossel are, it is understood, to be the future editors of this journal.

ARTHUR GAMGEE.

[1] Prof. Stokes, F.R.S., "On the Reduction and Oxidation of the Colouring-matter of the Blood." *Proceedings* of the Royal Society, vol. xiii. (1864) p. 357, paragraph 8.

[2] Hoppe-Seyler, "Erkennung der Vergiftung mit Kohlenoxyd." Fresenius' *Zeitschrift*, vol. iii. (1864) p. 439. *Philosophical Magazine*, vol. xxx. (1865) p. 456.

[3] "Um Verwechselungen zu vermeiden nenne ich den Blutfabstoff Hämatoglobulin oder Hämoglobin," Virchow's *Archiv*, vol. xxix. (1864) p. 233.

[4] Hoppe-Seyler's "Beiträge zur Kenntniss des Blutes des Menschen und der Wirbelthiere"; "Med-Chem. Untersuchungen," pp. 169-214, 366-385, 523-550; "Zur Chemie des Blutes und seiner Bestandtheile," *ibid.*, pp. 29-300; Dybkowsky, "Einige Bestimmungen über die Quantität des mit dem Hämoglobinlose gebundenen Sauerstoffs," *ibid.*, p. 117-132.

[5] "Medicinisch-Chemische Untersuchungen aus dem Laboratorium für angewandte Chemie zu Tübingen herausgegeben, von Dr. Felix Hoppe-Seyler." Berlin, 1866.

NOTES.

We are informed that a biography of Prof. Huxley is being prepared by his son, Mr. Leonard Huxley, who will be greatly obliged if those who possess letters or other documents of interest will forward them to him at Charterhouse, Godalming. They will be carefully returned after being copied.

The Committee of the Pasteur Institute have appointed Dr. Duclaux, formerly sub-director, to succeed M. Pasteur as director, and Dr. Roux to be sub-director of the Institute.

We understand that the final interment of M. Pasteur in the Pasteur Institute will not take place on Friday, as had been intended, because the vault and part of the sculpture cannot be ready in time.

The centenary celebrations of the Institute of France commenced as we went to press yesterday, and will terminate on Saturday by a visit to the fine chateau of Chantilly, where the associates and members will be received by the Duc d'Aumale. An account of the foundation and membership of the Institute appeared in these columns a few weeks ago, and we hope to give in our next issue a full description of the ceremonies now taking place.

A bronze portrait bust of Dr. Robert Brown was unveiled on Friday in his native town, Montrose, Forfarshire, at a reception held by the Provost, magistrates, and town council of Montrose. Beneath the bust is a tablet, with the following inscription:—"Robert Brown, D.C.L. Oxon., LL.D. Edinburgh, F.R.S. London, President of the Linnean Society, Member of the Institute of France. Born in this house 21st December, 1773; died in London 10th June, 1858. 'Botanicorum facile princeps,' Alex. Von Humboldt.'" A large number of distinguished botanists from all parts of the kingdom were present.

Mr. P. H. Lawrence, whose name will be remembered by some students of mineralogy, but more widely in legal circles, died a few days ago. We have also to record the death of Prof. E. W. Blake, until lately professor of physics in Brown University; of Dr. E. F. Rogers, instructor in chemistry at Harvard University; of Prof. V. Rydberg, the Swedish archæologist; of Mr. H. W. V. Stuart, who devoted much attention to the study of Egypt and its monuments; of Father Hirst, the author of numerous contributions to archæology; and of Dr. F. M. Stapff, the geologist, while prospecting for gold in Ea t Africa.

The sixth Congress of Medicine was opened at Rome on Tuesday by Dr. Baccelli, Minister of Public Instruction.

In addition to the papers, already notified in the usual way, to be read at the next meeting of the London Physical Society to-morrow, there will be read, if time permits, a paper "On the Magnetic Field of any Cylindrical Coil or Plane Circuit," by Mr. W. H. Everett.

The steamship *Windward*, which conveyed the members of the Jackson-Harmsworth Polar expedition to Franz Josef Land, arrived at Gravesend on Tuesday. It will be remembered that the *Windward* left the Thames in July 1894; she has brought back the records of the expedition from that date up to the beginning of July of this year. Mr. Jackson and his party remain in Franz Josef Land, and the vessel will return there, with stores, next June.

A Fine Art, Industrial and Maritime Exhibition will will be held in Cardiff in the spring and summer of 1896, under the patronage of Her Majesty the Queen. The general object of the exhibition is to illustrate the most recent progress in the sciences, arts, and manufactures, and not merely to be a popular show. The following is a list of the chief sections, and the number of square feet allotted to each:—Mining and mining appliances, 13,280; machinery, electricity, and local and general industries, 20,480; maritime, 8400; agriculture and horticulture, 7280; health, 5400; fine arts, 9600.

The annual exhibition of the South London Entomological and Natural History Society was held on Thursday last, and was much appreciated by the company who went to see the numerous interesting specimens arranged by the Committee. The Society has for its object the popularising of the study of natural history, and to promote this it holds bi-monthly meetings, at which papers are read, discussions take place, observations are communicated, and specimens shown and commented on. In the summer-time field meetings are held for the purpose of collecting and observing, and periodical exhibitions are promoted. The Society's rooms are at Hibernia Chambers, London Bridge, where a large library and typical collections are kept for members' reference, as well as a lantern for demonstration purposes. At present the number of members is about two hundred. The Secretary is Mr. Stanley Edwards, Kidbrooke Lodge, Blackheath, S.E.

Mr. D. Pidgeon, Letherhead, sends us an account of a curious effect apparently produced by lightning in the early morning of September 7. In a cottage on Cherkley Court estate, three or four tumblers were left standing over-night, mouth upright, on a shelf affixed to the wall of a small pantry, and about twelve inches from the window, which was open. In the morning one of these tumblers was found to have a crack completely round it, so that a ring of glass, having an uniform width of half an inch, could be cleanly and easily detached. This hoop of glass has been preserved to be a witness to the vagaries of electrical discharge. There seems little doubt that electricity had to do with the formation of the crack, for large shrubs, just outside the open window near which the glasses stood, were found to have been damaged by the lightning. It would be interesting to know whether the glass was empty or not, or whether it was wet up to the level of the crack.

The Harveian Oration was delivered on Friday last, at the Royal College of Physicians, by Dr. W. S. Church, who took for his subject "Harvey and the Rise of Physiology in England." For 239 years, with but few intermissions, the College has met in obedience to Harvey's direction to commemorate its benefactors. After referring to the long list of these, Dr. Church remarked that during the present year the College had received the magnificent endowment of £3000 to establish a triennial prize for the furtherance of original research on the prevention and cure of tuberculosis, the donor being Dr. Hermann Weber, who, in instituting the prize, joined the name of the late Dr. E. A. Parkes with his own. After the delivery of the oration, the Baly medal was presented by the President, Sir Russell Reynolds, to Dr. W. H. Gaskell, F.R.S., of Cambridge. The medal is awarded biennially to some person who has distinguished himself in the science of physiology; it was founded in 1866 by Dr. F. D. Dyster, "In Memoriam Gulielmi Baly, M.D.," and amongst the names of those who have since received it are those of Claude Bernard, Carl Ludwig, Darwin, Owen, Kitchen-Parker, and Brown-Séquard.

In connection with the proposal to change the name of the Boulevard de Vaugirard to Boulevard Pasteur, the Paris correspondent of the *Chemist and Druggist* points out that a Rue Pasteur already exists, while twenty-one other streets of Paris have been named after chemists. Of these fourteen were of French nationality, and include Chevreul, Gay-Lussac, Lavoisier, Raspail, &c. Davy figures as the sole English chemist, and the only other foreigner is the Swede Berzélius. The names of seven botanists appear on street corners, amongst which are Dupetit, Thouars, Jussieu and Linné. Nicholas Flamel, writer and alchemist, who flourished in the second half of the fourteenth century, has the distinction of being the most remote name connected with sciences after which the Parisians have called a street. Thirty-nine thoroughfares take their names from doctors and surgeons; amongst these figure Jenner and Vesale, the Belgian anatomist, the only two foreign names. We commend the French custom to English and municipal authorities at a loss for suitable street names. It may be thought a doubtful honour to have one's name handed down to posterity in this manner, but the custom serves to show that men of science are remembered in France in little as well as in great things.

The following statistics, from the *Zoologist*, with reference to the progeny of a female Manx Cat and an ordinary Tom Cat, are interesting. The successive litters consisted of three on each occasion, and the distribution of tails is shown in the table:—

	No tails.	Half tails.	Full tails.
1st litter	3	0	0
2nd ,,	2	1	0
3rd ,,	1	2	0
4th ,,	0	2	1
5th ,,	0	1	2
6th ,,	0	0	3

The gradual elimination of the tailless condition characteristic of Manx cats is singular, and well worth putting on record.

Very little detailed information exists as to the effect of wind and atmospheric pressure on the tides around the British Isles, but it is to be hoped that the Committee appointed at the recent meeting of the British Association will succeed in eliciting sufficient trustworthy data to enable some general law to be deduced for the guidance of navigators. The Committee consists of Prof. Vernon Harcourt, Prof. Unwin, Mr. G. F.

Deacon, and Mr. W. H. Wheeler (Secretary); and as it is desirous of obtaining information from as many ports as possible, we are asked to make its existence known. A printed form, showing the manner in which it is proposed to collect the tidal statistics, will be sent to any one who will render assistance to the Committee, by Mr. W. H. Wheeler, Boston, Lincolnshire, who will also be glad to receive records of tides affected by gales.

IN connection with the growth of orchids, writes Mr. J. H. Hart, in the October *Bulletin* of the Royal Botanic Gardens, Trinidad, it has been noticed that the presence of ants is apparently necessary to their maintaining a healthy condition; but whether this is in reality due to some action of the ant itself, or to some indirect cause, has not yet been proved, and investigations are needed to show what is the real influence the ant has upon the health of the plant. It has been suggested that the presence of stinging ants acts as a protection to the plants; but Mr. Hart is inclined to think, from recent investigations, that the benefit the ants confer on the plant are those of providing it with the mycelium of a fungus to cover its roots, which organism enables it to take up food which would be otherwise unattainable. It may be shown that the ants act as protectors to the plants, as well as providing them with a means of obtaining nutriment; but Mr. Hart believes it to be almost certain that the fungus which grows in the material they accumulate around the root plays a much more important part, by providing the plant with food material.

THE first number of what promises to be a useful serial publication has just reached us from the U.S. Weather Bureau. The periodical has for its name *Climate and Health*: it is edited, under the direction of Prof. W. L. Moore, the new chief of the Weather Bureau, by Dr. W. F. R. Phillips, and it is devoted to climatology in relation to health and disease. Tables are given showing, for one hundred selected stations, statistical information relative to atmospheric pressure, temperature, humidity, precipitation, wind, and sunshine; the relative prevalence of certain diseases; and the mortality from different causes, in each State. In addition to these statistics, all of which refer to the conditions during July of this year, the new publication contains charts showing the average pressure departures from the normal, ranges of pressure, prevailing winds, and normal wind directions for each week in the month, and similar charts to exhibit graphically the absolute and relative data referring to temperature, humidity, and precipitation. There is also a chart for each week showing the total mortality by States, and representing diagrammatically the average climatological conditions so far as determined by the mean temperature and humidities and the total amount of precipitation. The general aim of the Weather Bureau in this new field of work is to collect the meteorological and hygienic statistics considered by medical climatologists of the greatest correlative importance, and to publish them in a useful and instructive form. By showing the statistics of mortality and morbidity side by side with those of climate, new information as to connections between sickness and weather changes will probably be discovered.

THE *Psychological Review* for last month contains an interesting paper by Mr. R. Meade Bache, on "Reaction Time according to Race." He suggests that the higher intellectuality of civilised white races may have been gained at the sacrifice of quickness of response to sensory stimuli, and states that it is a matter of familiar observation that Negro children are quicker in their movements than the children of white folk. At his request Prof. Lightner Witmer made careful and exact observations on persons of the Caucasian, American Indian, and African (Negro) races. These are given in three tables. Taking response to auditory stimuli, for example, the order of quickness is (1) Indian, (2) African, (3) Caucasian, in the relation of 116·27 : 130 : 146·92; these being the reaction times in thousandths of a second. Although the numbers of individuals dealt with (not more than a dozen in each case) are small, the results are suggestive, and will no doubt lead to further investigation.

THE attention of those who are interested in the question of the inheritance of acquired characters may be drawn to a paper which Prof. Mark Baldwin contributed to *Science* (August 23, 1895), under the title "Consciousness and Evolution." Prof. Baldwin fails to see any great amount of truth in the claims of Mr. Spencer that intellectual progress in the race requires the hereditary transmission of acquired increments in mental faculty, and adopts the view advanced by Weismann in 1889, and taken up more or less independently by Mr. Ritchie and Mr. Kidd, that social advance is rather by tradition than by hereditary transmission. "Man," said Prof. Weismann, "availing himself of tradition, is able, in every part of the intellectual domain, to seize upon the acquirements of his ancestors at the point where they left them, and to pursue them further, finally himself leaving the results of his own experience and the knowledge acquired during his lifetime to his descendants, that they may carry on the same process." Prof. Baldwin seems to have reached this view independently, and his paper is well worth reading.

UNDER the extraordinary heading of "The Chemical Theory of Freedom of Will," Dr. W. Ostwald makes, in the *Leipziger Berichte*, some suggestive speculations upon the mechanical theory of the universe. That all the phenomena of nature, organic as well as inorganic, should be ultimately of a purely mechanical character, is contradicted by the science of energy. The theorems of energetics give the conditions under which any event takes place; they indicate which out of all the possible courses it will follow, and to what state of equilibrium it tends. All this does not involve the element of time, except in the case of kinetic energy. In the equations representing mechanical processes, time may be put as positive or negative without rendering them invalid. In other words, all purely mechanical processes are reversible, while natural processes are not. They have a forward and a backward aspect. Now there are processes in nature in which an agent influences the time during which a certain event takes place, without being itself affected in any way. This happens in all cases of catalysis, and the laws of catalytic action are as yet only very imperfectly understood. It is known, however, that the acceleration of the process is proportioned to the concentration of the catalyser. May not the human mind, the author argues, act upon matter somewhat in the manner of a catalyser, accelerating the chemical and mechanical processes associated with psychical activity without any expenditure of energy? This may be worth considering. But it must be remembered that the course of natural phenomena can be influenced in many ways without the expenditure of energy. An elastic missile rebounding from a rigid plane is a case in point, or a river flowing between its banks.

THE production of antiseptics appears to be more and more engaging the attention of the great German colour manufacturers, and yet another compound, rejoicing in the name of potassiumorthodinitrocresolate, has been introduced, which promises to prove of considerable service both to the brewer and to the horticulturist. Messrs. C. O. Harz and W. von Miller have published an account of their investigations with this substance—or *antinonnin*, as it is more generally called—in the *Muenchen Allgemeine Zeitung*, and it appears that a

solution containing but one part in 1500 to 2000 parts of soap-water proves destructive to all common injurious parasites without any deleterious action on the plants. Prof. Aubry, the well-known director of the experimental brewing station in Munich, has examined its disinfectant action on yeast, and finds that the latter, when treated with antinonnin, remained for a long time in a fresh condition in the heated workrooms, whilst untreated yeast rapidly underwent decomposition. A closer examination showed that all the specimens exhibited destruction of bacteria, while the yeast itself proved resistant to even stronger solutions, up to 5 per cent. Numerous other experiments have been made with this substance, and so far it promises well, being also odourless and very inexpensive. Whether this new antiseptic will succeed in carrying out all that is hoped of it, remains to be seen; meanwhile it may be regarded as an interesting, and possibly important, contribution to our list of disinfectants.

MESSRS. MACMILLAN AND CO. will issue in the course of November a further instalment of their "Cambridge Natural History." The volume is mainly devoted to insects, being the first part of a complete treatise on the subject by Mr. David Sharp, F.R.S. Introductory sections on Peripatus and on Myriapods are contributed respectively by Mr. Adam Sedgwick, F.R.S., and by Mr. F. G. Sinclair. The volume is the fifth in the series, and will be followed at no long interval by the second volume, in which various contributors deal with worms and Polyzoa. The ninth volume, in which Mr. A. H. Evans treats of birds, may be expected before the end of next year. Among Messrs. Macmillan's announcements for next week, one of the most important is that of an exhaustive work on "The Structure and Development of the Mosses and Ferns" (Archegoniatæ), by Dr. D. H. Campbell.

WITHIN the past few days, a bulky bundle of new publications of the U.S. Geological Survey has been added to the many reports and memoirs of the Survey already lying on our table. The amount of work represented by these volumes is so exceedingly great, that limits of space prevent us from attempting to describe and discuss the ground covered in them. We propose, however, to give in an early issue a general account of the recent publications of the Survey, and content ourselves at present with the bare statement of the volumes received during this month. First of all, we have to acknowledge the receipt of the fourteeenth annual Report of the Survey, in two parts. Part 1 contains the report of Mr. J. W. Powell, the Director, on the operations of the Survey for the year ending June 30, 1893, and part 2 (a volume of six hundred pages) contains papers on geological subjects, among which we notice—the potable waters of Eastern United States; the natural mineral waters of the United States; measurements of river discharges; the laccolitic mountain groups of Colorado, Utah, and Arizona; the gold-silver veins of Ophir, California; geology of the Catoctin Belt; tertiary revolution in the topography of the Pacific Coast; the rocks of the Sierra Nevada; pre-Cambrian igneous rocks of the Unkar Terrane, Grand Cañon of the Colorado. Two monographs of the U.S. Geological Survey have been received, viz. vols. xxiii. and xxiv. The former deals with the "Geology of the Green Mountains in Massachusetts," by Messrs. R. Pumpelly, J. E. Wolff, and T. Nelson Dale; and the latter contains Prof. R. P. Whitfield's text and drawings of the Mollusca and Crustacea of the Miocene formations of New Jersey. Both these valuable monographs are profusely illustrated. Finally, *Bulletins* Nos. 118-122 of the Survey have come to hand. No. 118 is a geographical dictionary of New Jersey. The next *Bulletin* contains the results of a geological reconnaissance in North-west Wyoming, with special reference to economic resources; No. 120 is on the Devonian system of Eastern Pennsylvania and New York. No. 121 is a bibliography of North American palæontology for the years 1888-92, inclusive; and No. 122 contains the results of the primary triangulation executed by the Survey during the past twelve years—that is, since the commencement of work upon the topographic atlas of the United States. In conclusion, we wish only to remark that the gratitude of geologists is due to the United States Government for providing the funds to publish so many works, not only of national but also of international importance.

THE current number of the *Journal de Physique* contains a paper by MM. Abraham and Lemoine on the measurement of very high potentials by means of a modified attracted disc electrometer. Two forms of instrument are described, the one for standard measurements, and the other, which is of simple design, intended for measuring potentials up to 100,000 volts to within about one per cent. In the standard instrument, which resembles a modified Kelvin electrometer as designed by M. Baille, the movable disc is suspended from the beam of a short-beam balance, the extent of the movement being limited by stops. In order, when desired, to make the movement of the balance beam stable, an auxiliary knife-edge is placed below the chief knife-edge of the beam, and weights are placed in a pan suspended from this auxiliary knife-edge. The attracted disc is maintained centrally within the guard-ring by means of three fine fibres. The simplified form of electrometer is, however, the one which exhibits most novelty. In this instrument the attracted disc is carried by a rod attached to one arm of a Roberval's balance. The movements of the balance, which is limited by stops, is noted by means of a long pointer attached to one of the horizontal bars of the moving parts. Finally, the adjustments of the guard-ring and attracted disc are not made by means of a complicated system of adjusting screws, but by the simple bending of their supports. These supports are made of soft copper wire, and, in the case of the guard-ring, have an S shape. This manner of allowing for the adjustment of the parts of a piece of apparatus is one which will very often be found of use, and we may mention that lead wire is particularly well suited for the purpose. The authors have made a series of experiments to test what is the maximum distance between the attracted and attracting discs it is allowable to use, and find that the greatest distance to be equal to half the width of the guard-ring. In making their measurements, the authors have used a novel method of obtaining a high potential which should remain steady for some minutes. Their arrangement consists of an electrostatic electric machine driven at a uniform speed by a small motor. The poles of the machine are joined to two points, between which a continuous stream of sparks passes. One of these points is connected to earth, and the other by means of a poor conductor, such as cotton soaked in paraffin oil, to the inner coating of a Leyden jar. Under these circumstances it is found that the potential of the interior coating of the jar is very constant. Thus in a series of measurements recorded by the authors, the maximum change in six minutes amounted to only 1 part in 1000, the potential being about 20,000 volts.

THE additions to the Zoological Society's Gardens during the past week include a Mozambique Monkey (*Cercopithecus pygerythrus*, ♀) from East Africa, a Smith's Dwarf Lemur (*Microcebus smithi*) from Madagascar, presented by Mr. E. Dyer; a Rhesus Monkey (*Macacus rhesus*, ♀) from India, presented by Mrs. Vernon Biden; a Polar Bear (*Ursus maritimus*, ♂) from Spitzbergen, presented by Mr. Arnold Pike; two Masked Parrakeets (*Pyrrhulopsis personata*) from the Fiji Islands, a Blue and Yellow Macaw (*Ara ararauna*) from South America, a Peregrine Falcon (*Falco peregrinus*, var. *Anatum*) from North America, a Night Heron (*Nycticorax*

griseus), European, an Antarctic Skua (*Stercorarius antarcticus*) from the Antarctic Seas, presented by the Hon. Walter Rothschild; two Senegal Touracous (*Corythaix persa*) from West Africa, presented by Mr. I. J. Roberts; three Blackcaps (*Sylvia atricapilla*), a Nightingale (*Daulias luscinia*), British, presented by Mr. Poynter; a Wall Lizard (*Lacerta muralis*) from Sicily, presented by Mr. A. M. Amster; a Dwarf Chameleon (*Chamæleon pumilus*) from South Africa, presented by Mrs. S. Jackson; two Squirrel Monkeys (*Chrysothrix sciurea*) from Guiana, a Spotted Eagle (*Aquila nævia*) from India, three Weka Rails (*Ocydromus australis*), four Tuatera Lizards (*Sphenodon punctatus*) from New Zealand, deposited; two Grisons (*Galictis vettata*), a Coypu (*Myopotamus coypus*) from South America, two Western Boas (*Boa occidentalis*) from Paraguay, purchased.

OUR ASTRONOMICAL COLUMN.

Sun-spot Observations in 1894.—In a *Separatabdruck aus der Vierteljahrschrift der Naturforschenden Gesellschaft in Zürich*, Jahrgang 4, 1895, Dr. A. Wolfer brings together some results relating to the sun-spot statistics made in Zürich and elsewhere for the year 1894. The pamphlet opens with a determination of the constants for reducing the observations of each observer to one scale.

The mean observed relative number of spots for 1894 came out as 78·0 as against 84·9 in 1893, showing a distinct decrease. The secondary variations were also very prominent during this year; further, between two very low minima there occurred a prominent maximum lasting from May to July. Nevertheless there was on the whole a general decrease, making it possible to determine the epoch of the last important maximum. Having plotted the relative number of observed sun-spots for the three years 1892–94, and connected them together, the smoothed curve indicated a maximum at 1894·0. The length of the elapsed period, that is, from maximum to maximum, became

$$1894{\cdot}0 - 1883{\cdot}9 = 10{\cdot}0,$$

and the interval between the last minimum and the present maximum

$$1894{\cdot}0 - 1889{\cdot}6 = 4{\cdot}4.$$

Dr. Wolfer makes a comparison of the sun-spot numbers with the variations of the magnetic declination. Here there seems to be a very good agreement, and the curves for both are very similar. The epoch of the maximum magnetic variation, independently determined, occurs in August 1893 or 1893·6, which coincides exactly with the secondary rise of the curve of relative spot numbers. This secondary rise in the curve occurs just before the time of maximum deduced from the smoothed curve, and suggests rather that the former date should represent the chief sun-spot maximum. Dr. Wolfer, however, is not of this opinion, and prefers to hold to the date gathered from the mean curve. The pamphlet concludes with a tabular statement of each of the observers' individual observations for the year 1894, together with reference to the literature.

Planetary Perturbations.—In No. 3312 of the *Astronomische Nachrichten*, Prof. A. Weiler gives another paper on the subject of long-period and secular perturbations. The particular case considered is that of the disturbance of a planet, having a mean motion approximately twice that of the disturbing planet, and is really a special case of the more general problem of perturbations already treated in earlier numbers of the same journal. We cannot indicate here the mathematical formulæ which are given, and much of which would be unintelligible without the earlier papers, but attention may be called to one of his results.

When the commensurability in the periods of the disturbed and disturbing planets becomes very close, that is if $\delta = 1 - 2\mu$ be very small, where μ is the ratio of the two mean motions, the series by which the perturbations are expressed is not convergent, and the problem is apparently insoluble. Such a result is inconsistent with the regularly observed motions of the planets, and therefore points to some error in the assumptions on which the solution of the problem is founded. This error Prof. Weiler traces to the treatment as constant of the semi-axis major of the disturbed planet's orbit. The justice of this remark is illustrated by a reference to the arrangement of the asteroids in space, whose distribution offers peculiarities explicable on the hypothesis that the mean daily motion is variable if the approximation to commensurability oversteps a definite limit. Taking a list of twenty-five asteroids, wherein the value of $\delta = 1 - 2\mu$ is less than one-fifteenth, he shows that none have a period giving a mean daily motion very approximately twice that of Jupiter ($598''{\cdot}3$). The mean daily motion of these twenty-five lies between $562''{\cdot}2$ and $640''{\cdot}2$, but none come between $572''{\cdot}6$ and $614''{\cdot}4$; that is, the mean motions separate on both sides of twice that of Jupiter. The force of this illustration is somewhat impaired if the list be made to comprise those more recently discovered. The asteroids Nos. 332 and 381 have mean motions of $605''{\cdot}5$ and $613''{\cdot}5$, respectively, and it should further be remembered that in the whole list of asteroids, there are only five whose means approach the lower limit of $562''$. This remark simply refers to the value of the illustration, not to the accuracy of the fact it is called in to support.

The System of α Centauri.—The meridian measures of the positions of α_1 and α_2 Centauri, made at the Cape in 1879–1881 have been utilised by Mr. A. W. Roberts for a determination of the relative masses of the two stars, and other data connected with the system (*Ast. Nach.* No. 3313). The place of the centre of gravity for 1880 is given as R.A. 14h. 31m. 27·537s., declination $-60°\ 20'\ 20''{\cdot}63 \pm 0''{\cdot}13$; proper motion in declination (1880) $= +0''{\cdot}750 \pm 0''{\cdot}005$; proper motion in R.A. (1880) $= -7''{\cdot}291 \pm 0''{\cdot}032$. For the relative masses of the two stars, the values derived are 51 to $49 \pm 1/50$ of the amount.

According to the results obtained by Mr. Roberts, α_2 Centauri is very slightly heavier than the sun, while α_1 is about two-hundredths lighter. Since α_2 is now between five and six times brighter than α_1, it must have by far the brighter surface. Taking a mean of the different values which have been obtained for the sun's brightness in relation to the stars, "it would appear that α_2 Centauri is as bright as our sun, while α_1 is about five times fainter. α_1 Centauri is accordingly some distance on the downward track from the dignity of a sun to that of an ordinary planet; while α_2 Centauri is, as regards light, size, and mass, a twin-brother of our sun." Spectroscopic observations will furnish another method for determining the relative masses, but, in order to improve on our present knowledge, the observations of velocities must be accurate to within one or two tenths of a mile per second.

Holmes' Comet.—This comet, which has presented such peculiarities both in its physical structure and the form of its orbit as to make it one of the most remarkable comets of short period, has been made the subject of an elaborate investigation by Dr. H. J. Zwiers. Taking into account the action of Jupiter and Saturn, but neglecting that of the Earth, to which, owing to the great perihelion distance of the comet, it cannot make any close approach, Dr. Zwiers is led to fix the date of the next perihelion passage on April 27, 1899, and gives an ephemeris commencing on February 16, 1898, the earliest date at which a search is likely to be successful. The theoretical brilliancy is then 0·0063, and when last seen in 1893, the brilliancy was expressed by 0·0118. In April and May, when the comet will be well situated for observation in the southern hemisphere, this latter quantity will be exceeded, and will approach that, that the comet possessed in January 1893, when it underwent such a remarkable change in its appearance. If the comet retains its stellar-like character, the difficulty in detection will no doubt be increased, but an early discovery is eminently desirable.

ON THE HABITS OF THE KEA, THE SHEEP-EATING PARROT OF NEW ZEALAND.

THE kea, the mountain parrot of New Zealand (*Nestor notabilis*), has earned considerable notoriety from its remarkable habit of attacking living sheep. It is commonly stated that the natural food of this bird consists of insects, fruit, and berries; and that it has developed a taste for a carnivorous diet only during the last thirty years. Mr. Taylor White, however, has recently pointed out (*Zoologist*, August 1895) that the various statements on the habits of this bird have all been derived from second-hand information; and, as the habitat of the parrot is on the tops of Alpine ranges, owners of sheep and shepherds who

in winter and summer search the mountain tops for their stock, are the men best fitted to tell us about the habits of the bird. On observations made during such experiences Mr. White bases his own account. In the district with which this writer was acquainted, the kea always lived high up on the mountains, among rocks and boulders, a long distance above the forest-line; in such a situation, of course, berries and fruits were out of the question, and the bird appeared to live on lichen and any insects it could find. Even when the ground was covered with several feet of snow, and when roots and other food were out of reach, lichen growing on steep rocks would still be obtainable by the bird. The view that the diet of the kea generally consists of fruit and berries would thus appear to be erroneous.

It will be remembered that Wallace and others state that the kea regards the kidneys of sheep as a "special delicacy," and that it attempts to burrow into its victim in such a way as to reach this part. Mr. White, however, opposes this prevalent view, and regards it as probable that the bird desires to obtain the blood of the sheep rather than the kidneys; and in support of this view states that he has never seen a dead sheep attacked by keas. The fact that the kea so frequently pierces the body of a sheep in the region of the kidneys is due to the position it takes on the back of its victim to maintain a firm hold—a position from which it cannot be easily dislodged, as it could from the head or rump of the sheep. In corroboration of this Mr. White mentions that sheep with long wool are more frequently attacked than animals with short wool; as apparently the long wool gives the bird better facilities for holding on with his feet when drilling a hole into the back of the sheep. It is not very easy to conjecture how this habit of attacking sheep was first acquired by the kea. In winter time the sheep are covered with snow, and often have icicles hanging to their wool; and it is suggested by Mr. White that keas may have mistaken sheep so disguised for snow-covered patches of rock. It may further have happened that when searching the supposed rocks for insects the birds in some cases would taste the blood of the sheep. "When some of the birds had once found out that the blood of the sheep was good for food, others were soon initiated into the performance." It is possible that in some such manner the kea may have gradually acquired this curious and unattractive habit which renders the bird such a pest to the New Zealand farmer.

W. GARSTANG.

THE PENETRATION OF ROOTS INTO LIVING TISSUES.

THE capacity possessed by the roots of certain parasites, such as *Cuscuta*, to penetrate into the tissues of their host, is apparently an unique, not to say a remarkable phenomenon. A little reflection, however, upon the powers of roots in general, leads us to doubt whether this property is really as restricted as the first glance would lead us to imagine; and when we peruse Prof. Pfeffer's work upon the pressure of the root, and find that, for instance, the root of the common bean exerts during its growth a pressure of some 400 gms., we realise that this mechanical action alone might suffice to drive the growing root of most plants into living tissue, if circumstances necessitated such an expediency. This is evidently an important point, and touches upon the evolution of the higher parasites; it is only remarkable that it has so long remained untouched. We must now thank George Peirce for taking up this neglected subject, and placing it upon a sure basis (see *Bot. Zeit.* September 1894). The question first to be decided was whether the pressure which Pfeffer had found in the growing roots was in itself sufficient to force the roots through living tissue. For the determination of this, iron models of roots weighted up to 270 gms. were employed. The apices of these were placed upon a cube cut from a potato, and the whole surrounded with damp sawdust to keep the living substance fresh. After an interval of twenty-three hours, it was found that the iron point had penetrated $1\frac{1}{2}$ m.m. into the potato. Again, a similar model weighted to 320 gms. was driven in twenty-four hours through the cork layer and 2 m.m. of parenchyma of an uncut potato. Also a root-model placed on the stem of *Impatiens sultani*, one and a half centimetres thick, pierced this in less than twenty hours when 300 gms. weight were employed.

Thus a pressure inferior to that found by Pfeffer in the root of *Vicia faba* was sufficient to drive an iron model an appreciable distance through the living tissues of the potato.

It was far from certain, however, whether a pressure which was ample to impel a rigid iron rodlet against a considerable resistance would have equal efficiency in the case of a root, the pressure in which arose from so uncertain and inextricable a source as its life.

There were many facts both *pro* and *contra*.

The acid substance or substances, which it would seem that most roots excrete during their growth, might possibly facilitate the root's power of penetration. Just as many fungi eat their way, as it were, into the solid wood of their host by means of ferment-like substances which they secrete and pour out upon their substratum, so might the roots perhaps be expected to soften and prepare their way by means of their acid excretions. Against the supposition could be raised the fact, already broached, that the forces, impelling the root-apex forward, are derived from the vital activities of that structure, and than these nothing can be more sensible to change of surroundings, or less to be reckoned upon by us, whose conceptions of anything dealing with life are yet shrouded over with the darkest obscurity.

But to pass from speculation to facts, we find that Peirce tested this point by experiments on the seedlings of *Brassica napus* and *Sinapis alba*. He took a potato, and split it in half; on one of the halves he cut a number of small slits, into each of which he inserted a seed of the plant under observation. He then placed the potato-halves together, binding them tightly with string. The whole contrivance was placed in a vessel containing damp sawdust, care being taken that the cut surfaces of the tuber lay horizontally. After an interval of twelve days the specimens were examined, and although some were found to have grown between the cut surfaces (for nearly all had germinated), yet others had pushed their rootlets vertically downwards so as to penetrate the substance of the potato. In some instances so vigorous had been the growth that the rootlet had traversed the whole thickness of parenchyma, pierced the hard corky layer of the surface, and then reached the sawdust without.

Anatomical examination of the root and surrounding potato tissue showed several peculiarities. In the first place, the young root was almost devoid of the customary clothing of hairs; secondly, the cells of the potato had undergone alteration, inasmuch as those which were in immediate contact with the advancing root were much contorted and torn, whilst two or three layers neighbouring on the injured elements had undergone division by walls parallel to the long axis of the root, and had subsequently become corky in nature. By this means the intrusive rootlet was enclosed within a corky cylinder or sheath, cutting it off more or less perfectly from the living, unharmed tissue of the tuber. The starch grains were in every case unaltered, but Prunet, in his research on *Cynodon*, and Peirce, in his examination of one of his specimens of *Pisum*, noticed certain grains in the neighbourhood of the root apex which were partially disintegrated. This, however, is not a necessary consequence of ferment action; indeed, a check experiment of Peirce's leaves little doubt that the disintegration results in these cases from the activities of bacteria which had gained an entrance with the root. Glass tubes closed and pointed at one end were sunk, like the iron models already mentioned, into potato tissue. In one instance the apex of the glass was surrounded by "corroded" starch-grains. Here there could be no question of ferment formation, and evidently bacteria were adherent to the apex.

"So far the experiments had proved that the thin, delicate, and pointed roots of rape and white mustard are able to penetrate living tissues. Peirce carried the matter further by testing the powers of the blunt rootlets of *Pisum* and *Vicia faba* to do likewise. The rootlets of germinating seeds of these were placed in glass tubes into which they accurately fitted, and their apices placed in contact with the surface of a cube of potato. The seed and glass tube were rigidly held by layers of gypsum, in which a gap was left for the extension of the plumule. The whole was kept moist by damp sawdust. After three days the roots were found to have pierced the living tissue to the extent of 7·5 m.m.

Other experiments were made on the same plants in which other tissues, such as stem of *Impatiens sultani*, leaves of Echevaria and Aloe, petioles of Rheum, &c., were substituted for the potato. These also were penetrated by the rootlets.

In some instances, however, such as leaves of Aloe and petioles of *Rheum officinale*, the pabulum was evidently unsuited to the healthy existence of the root, for after a short

period of growth the apex of this organ became more or less spherical, and finally withered away.

Similar results had been obtained with the haustoria (modified roots) of Cuscuta, in a former research of George Peirce's.

Another interesting achievement of the same worker was to grow specimens of Pisum as parasites upon other plants, from the seedling stage until flowering. The host which gave the most favourable results appears to have been *Impatiens sultani.*

The young Pisum grown under these unwonted conditions produced an almost normal root system, with numerous side branches; but the stem was stunted in its growth, although it bore leaves and a few flowers. The roots, it may be mentioned, were here also devoid of hairs. This experiment is extremely interesting in a great many ways. It shows, in the first place, how fine is the line of demarcation between an ordinary earth-grown plant, such as Pisum, and a phanerogamous parasite, especially a partial parasite like mistletoe.

Again, it has a physiological interest; it is suggestive of a new path of research. A strict and careful comparison of the details of outward form and internal anatomy in a normally grown Pisum, or other plant, with those found in one which is, so to speak, an induced parasite, must, beyond all doubt, shed much light upon the relationship between the shape and structure of an organism and its surroundings.

We know but too little of this branch of biology at present.

Why an organ should be shaped this way in one individual and that way in another, may indeed be partially answered in some cases; but these instances are few, and the answers are incomplete, to say the least of them. RUDOLF BEER.

DR. A. SCHMIDT'S THEORY OF EARTHQUAKE-MOTION.

[NOTE.—The following pages contain a summary of an interesting but little known paper by Dr. August Schmidt, of Stuttgart. An English translation was prepared by the late Dr. E. von Rebeur-Paschwitz for the *Seismological Journal of Japan*, but arrived too late for publication in the concluding volume of the series. The original being too long for insertion in NATURE, I have condensed it at the translator's request, at the same time adhering as closely as possible to the author's words. The title of the paper is "Wellenbewegung und Erdbeben ein Beitrag zur Dynamik der Erdbeben" (*Jahreshefte des Vereins für vaterl. Naturkunde in Württemburg*, 1888, pp. 248-270). In a later paper (same journal, 1890, pp. 200-232), Dr. Schmidt applies his theory to the Swiss earthquake of January 7, 1889, and the Charleston earthquake of August 31, 1886.—C. DAVISON.]

SEISMOLOGISTS assume the propagation of earthquake-waves to take place uniformly in all directions; they regard the coseismal or wave-surfaces as concentric spheres, the rays as straight lines normal to the spheres. This, however, is an entirely unjustified assumption, which certainly facilitates the calculations, but leads to very doubtful results in determinations of the velocity of propagation and of the depth of the earthquake-centre. It is impossible that seismic rays should be straight lines, because the conditions on which the velocity depends undergo change with increasing depth below the surface. Though experimental determinations of the velocity do not agree with the theoretical value $\sqrt{e/d}$, yet it is clear that the velocity must depend on the density and elasticity of the rocks through which the wave is propagated. Now, the modulus of elasticity, owing to increased pressure, must increase with the depth below the surface; and therefore the velocity of the earthquake-wave must also increase with the depth.

As the velocity of propagation increases, the energy of a vibrating particle diminishes; and thus, as is well known to be the case, earthquakes should be less noticeable in mines than on the surface of the earth.

Amendment of Hopkins' Law.—Let us imagine a wave emanating from a deep centre and propagated in all directions. A vertical plane through the centre cuts all the successive coseismal surfaces, as well as the earth's surface. Let us suppose the section of the latter to be a horizontal straight line. The lower parts of Figs. 1 and 2 show the successive positions of the coseismal surfaces from minute to minute. Fig. 1, with its equidistant concentric coseismals and its straight rays, corresponds to the ordinary earthquake theory. Fig. 2, with its excentric coseismals approaching each other as they rise and with its curved rays convex downwards, represents our new theory. The horizontal straight line, dividing the upper part of the figures from the lower, represents the surface of the earth. In both figures, the rays at first appear equally distributed in all directions from the centre; in Fig. 1 they remain so, but in Fig. 2, in order to continue normal to the wave-surfaces, they must diverge at a much more rapid rate below than above, thus becoming convex downwards. Of course, Fig. 2 only represents a special law of increase of velocity with the depth—the velocity is supposed to vary as the depth—but the general character of the figure with its rays convex below remains the same if the law is a different one.

A comparison of the figures shows that in Fig. 2 there is a greater condensation of the seismic rays, and therefore a greater intensity of the shock, in the neighbourhood of the epicentre, and this corresponds better with the effects observed within the area of greatest disturbance.

But more important for our purpose are the sections of the earth's surface contained between two successive coseismals. Each of these sections is a measure of the distance through which the wave appears to progress from minute to minute at the surface. In reality it progresses obliquely from below in the direction of the rays, and the real distance through which it moves is smaller than the apparent one. We can only observe the apparent velocity at the surface. If we have at our disposal a sufficient number of good time-observations, we can draw the horizontal coseismal lines on a map and determine the apparent velocity from their relative distances. In both figures, the apparent velocity has its greatest value at the epicentre and decreases outwards. In Fig. 1, it gradually approaches asymptotically the true value in the direction of the rays. This is the law which Hopkins propounded in 1847. In Fig. 2, the apparent velocity at first diminishes rather rapidly, until it reaches the value of the true velocity at the depth of the centre, but afterwards it again increases gradually with the distance. We thus arrive at the following amendment of Hopkins' law, which will be expanded afterwards: the apparent velocity at the surface is never less than the true velocity at the centre, and varies with it.

Differences in Earthquake Velocities.—According to the old theory, every substance ought to possess its own velocity, dependent on its internal structure. The limit, which is defined by Hopkins' law as the lowest possible value of the apparent velocity, ought always to be the same in any given region. Experiments by Pfaff, Mallet, and Abbot lead to different values for different substances, as was to be expected. But they also show considerable variations in the same material, the velocity increasing with the strength of the initial impulse. Real earthquakes show even larger differences in velocity than artificial ones, and often earthquakes of less intensity are propagated with a greater velocity in the same region than very strong ones.

These differences are inconsistent with Hopkins' law. To be explained by the old theory, they require for the centres of earthquakes with great velocities an enormous depth below the surface, a near approach to the centre of the earth, for an earthquake emanating from the centre itself would arrive simultaneously at all points of the surface. With our new hypothesis, such differences are necessary, and even with the largest velocities the earthquake-centre may be at a considerable distance from the centre of the earth.

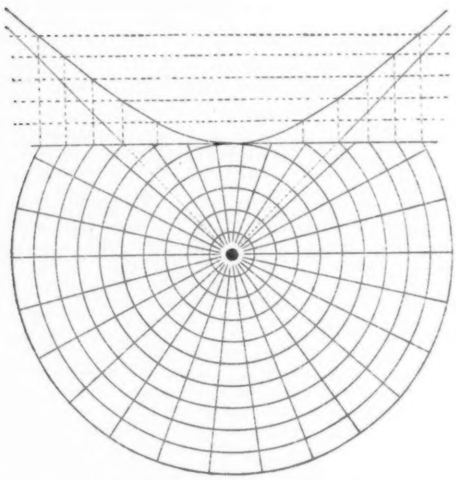
Proof of the Law.—The law that the velocity at the surface is never less than that at the earthquake-centre includes Hopkins' law. This indicates that the law is a general one. Its mathematical demonstration is contained in the law of refraction. We may distinguish the following three velocities: (1) the velocity at the centre, u_1; (2) the true velocity at the surface, *i.e.* that part of an earthquake-ray through which the wave progresses in one minute, u; (3) the apparent velocity at the surface, *i.e.* the horizontal distance between two successive coseismals corresponding to an interval of one minute, v. As an example, let us take in Fig. 2 the horizontal distance between the fourth and fifth coseismals from the epicentre as a representative of v, and let the section of the ray between the same coseismals near the surface represent u, and the distance between the centre and the first coseismal u_1. Then, if α be the angle between the ray and the vertical at the point where it meets the surface, we have $v = u/\sin \alpha$; and, if α_1 be the angle which the same ray makes with the vertical through the earthquake-centre, we have by the law of refraction $v = u/\sin \alpha = u_1/\sin \alpha_1$.

Now, let us consider the different rays emanating from the

earthquake-centre. When α_1 is equal to zero, v is infinitely great. As α_1 increases, v decreases, until $\alpha_1 = 90°$. This corresponds to the ray which starts horizontally from the centre, and at the point where this ray reaches the surface we have $v = u_1$. When α_1 becomes obtuse, the value of sin α_1 decreases again, and v increases, though more slowly because the rays diverge more and more; but at an infinite distance v would again be infinitely great.

The only condition by which our law is bound is that the true velocity of the wave is always the same at the same depth; but the variation of velocity may follow any law. The law would even remain true if the velocity were to decrease with the depth; but in this case the rays would be concave downwards, and only a few would reach the surface. But, as we have every reason to believe that v increases with the depth, it follows that the rays must be convex downwards; and not only the ray which is horizontal at first bends upwards, but all rays do so in time. The whole disturbed area of an earthquake is thus divided into two zones: an inner circle in which the apparent velocity v decreases as the distance from the epicentre increases, and an outer ring in which v increases with the distance up to infinity. The inner circle is the region of the direct rays, the outer ring that of the earthquake energy which by refraction is brought up from below. The smallest value of v, corresponding to the boundary between the two zones, measures the velocity of propagation at the depth of the centre.

Fig 1

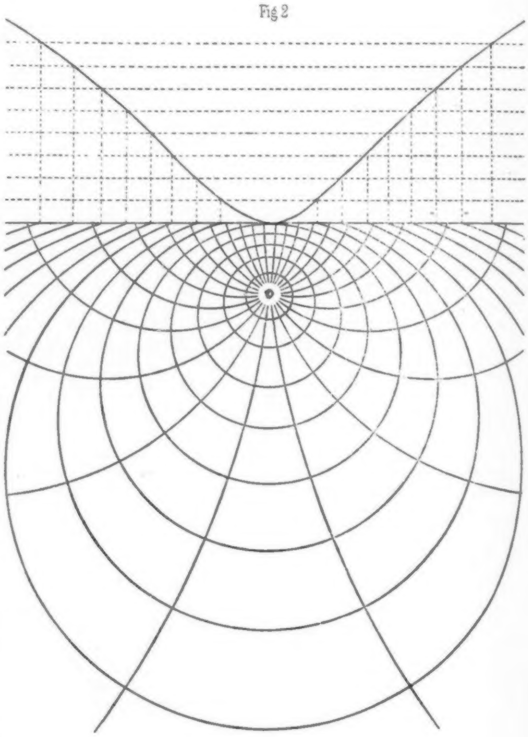


The effect of curvature of the earth's surface, which we have so far neglected, will consist in a diminution of the rate at which the velocity increases in the outer zone.

The Earthquake Hodograph.[1]—The law connecting the variations in the apparent velocity at the surface is best explained by the upper parts of Figs. 1 and 2. At the points where the surface line is cut by the coseismals, normals are erected to the surface of 1, 2, 3, &c., units in length, representing the differences in time from that at the epicentre. A curve passing through the ends of these normals represents what we call the hodograph. The greater the inclination of the curve to the surface line, the less is the apparent velocity, v, at the corresponding point of the curve. Where the curve is horizontal the velocity is infinitely great, where it is convex downwards the velocity decreases outwards, where it is concave the velocity increases. The hodograph in Fig. 1 is the hyperbola of von Seebach and Minnigerode. If we use the same scale for the units of time and velocity, the hyperbola is rectangular and the asymptotes are directed towards the centre. In Fig. 2, the hodograph is no longer hyberbolic; at the epicentre it is horizontal and convex downwards, gradually approaching a maximum inclination at a point of inflexion, after which it becomes concave downwards, and gradually becomes horizontal again at infinity. If, in the lower part of the figure, we follow the ray which leaves the centre horizontally until it reaches the surface, a normal erected at this point passes through the point of inflexion.

It is important to study the changes in the form of the hodograph as the depth of the centre gradually diminishes. The result is that the two points of inflexion move towards the epicentre, the convex portion becomes smaller, and so also does the "inner zone" of the disturbed area. When the centre and epicentre coincide, the convex portion of the curve and the inner zone of the disturbed area disappear entirely: the hodograph consists of two symmetrical concave branches which meet at an angle at the centre. This suggests to us how we should explain the results of measurements of velocities in artificial earthquakes. In a shock produced at the surface of the earth, the velocity must increase from the centre outwards. The stronger the charges of gunpowder are, the longer are the distances that can be employed in the experiments, and the greater the mean values of the velocity obtained.

Fig 2



Thus, the form of the hodograph will vary much with the depth of the centre, and it must also vary with the law which expresses the change of velocity with the depth. But, whatever be the unknown law, the hodograph must always be convex at the epicentre, and, passing through a point of inflexion, afterwards become slightly concave. This follows simply from the law of refraction without any regard to the rate at which the velocity increases with the depth.

As long as we do not possess a sufficiently large number of time-observations for at least one earthquake, it will be impossible to draw any conclusion concerning the law of velocity from the form of the hodograph. Even with the best observations, we can never, in drawing the hodograph, expect that all points will fall on a regular and continuous curve. But what we may expect is that, with a sufficiently large number of observations, the points will be distributed equally on both sides of such a curve. The hodograph contains the observations from places

[1] The name "hodograph" was given by Hamilton to a curve which represents graphically the variable velocity of a moving point. We do not think that any mistake can arise if we use this name for our purpose.

in all possible directions from the epicentre combined in a single plane. If the velocity is different in different directions, in the general figure these differences will be eliminated when the number of observations is large enough, and the result will be a curve free from local disturbances.

Although the time has not yet come for us to determine the definite form of an earthquake-hodograph, yet we know enough from the best examined earthquakes to decide whether the hodograph is an hyperbola or a curve with points of inflexion, whether Hopkins' law is confirmed by the observations, or an increase of velocity is noticeable in the outer zone of the disturbed area.

The best example for such an investigation is contained in von Seebach and Minnigerode's discussion of the earthquake of March 6, 1872, in Central Germany. An inspection of the map of coseismals published by them is sufficient to show that the horizontal coseismals are crowded together in a striking manner near Göttingen and Leipzig, at a distance of sixteen (German) miles from the epicentre. Accordingly, in drawing the hodograph we see how badly the hyperbola suits the observations. Several points which are most valuable for the determination of the epicentre, because they are nearest to it, and which agree most perfectly with one another, must be rejected in constructing the hyperbolic hodograph, in order that the earthquake may not begin at the surface of the earth until $1\frac{1}{2}$ minutes after it was actually observed at five different places at five to six miles distance from the epicentre. For sixteen miles the hyperbola leaves all the best observations below it, after which nearly all points remain above it until it ends at Breslau, at a distance of fifty-seven miles from the epicentre. At this place a magnetic needle was found swinging by Prof. Galle at 4h. 5m. 25s., Berlin time, but the shock itself may have occurred several minutes earlier. The hyperbola is made to pass exactly through the point corresponding to this time, for otherwise its vertex would have to be placed still higher than it is now, and this would increase the already existing disagreement between the calculated time of the beginning of the earthquake and the actual observations.

How well, on the contrary, are the observations represented by a curve the vertex of which is a little below 3h. 55m., and, being convex downwards, passes at a distance of seven to eight miles between 3h. 55m. and 3h. 56m., reaches its points of inflexion at about eleven miles distance with a slope corresponding to ·2·5 miles per minute, and then leaving some points on one side and some on the other, passes through Tübingen (36·7 miles), the last trustworthy point, until it reaches Breslau one minute before the observed time, with a velocity of at least fifteen miles a minute.

The Herzogenrath earthquake of October 22, 1873, leads to somewhat similar results. In drawing the hyperbolic hodograph, some of the best observations, those used for determining the position of the epicentre, have to be rejected altogether, while others must be supposed to err by as much as two or three minutes. But a curved line, passing through the mean positions of the points, is concave throughout on its lower side, with a large curvature at the epicentre. The figure certainly differs little from the form of the hodograph corresponding to a centre at the surface, and the inner zone is a circle of not more than four kilometres radius.

Thus the best investigated earthquakes at our disposal show that the observations agree much less closely with the older theory of concentric earthquake-waves, straight rays and hyperbolic hodograph, than they do with the new theory of a velocity of propagation increasing with the depth, rays convex downwards, and a hodograph with points of inflexion.

The Determination of the Depth of the Centre.—If the law connecting the velocity with the depth were known, we should be able to calculate the forms of the corresponding rays and hodograph, and to find a relation between the depth of the centre and the form of the hodograph. In Fig. 2 we have started with the simplest assumption, and supposed the velocity to vary as the depth. As this law is an entirely arbitrary one, the figure can only give a nearer approach to the truth than the theory represented in Fig. 1. If, for instance, the modulus of elasticity were to vary as the depth, the velocity would change much more rapidly near the earth's surface than far below it; and the fact that the perceptibility of earthquakes decreases so rapidly as the depth increases, certainly indicates that a rapid change in the velocity takes place immediately below the surface. Consequently, in calculating the depth of the centre according to our law, we should find too large a value. Other difficulties in determining the depth of the centre are our ignorance of the true superficial velocity, and the uncertainty as to the form of the hodograph, especially the doubtful position of its points of inflexion. But, in spite of all these difficulties, we may consider it as a rule that the depth will increase with the radius of the inner zone of the disturbed area, and that it will certainly always be smaller than this radius.

On the other hand, a minimum value of the depth may be found by means of the tangent at the point of inflexion. This tangent in Fig. 2, like the asymptote in Fig. 1, makes an angle of 45° with the horizon, because in both figures the central velocity (u_1) was taken as the time scale. While in Fig. 1 the asymptote passes through the centre, in Fig. 2 the tangent at the point of inflexion passes above it. Now, let us imagine the depth of the centre in Fig. 2 to remain the same, as well as the velocities u_1 at the centre, and u at the surface; but let the increase of velocity be no longer uniform as before, but be principally restricted to the neighbourhood of the surface. The consequence will be that the rays will differ little from straight lines at first when they leave the centre, and that the principal increase of curvature will be near the surface. The point of emergence of that ray which leaves the centre horizontally, will move to a greater and greater distance, and, as the same is the case with the point of inflexion of the hodograph, its tangent at that point will be displaced parallel to itself downwards; and when the whole change is imagined to take place in the surface itself, the hodograph will coincide with Seebach's hyperbola, and the tangent at the point of inflexion becomes an asymptote and passes through the centre.

Thus, with a hodograph adapted as well as possible to the existing observations, we find a depth of more than five, and less than ten, geographical miles for the earthquake in Central Germany, and a depth of less than three kilometres for the earthquake of Herzogenrath. Each of these earthquakes represents a special type. Type I., with a very small depth of centre and an approximate disappearance of the inner zone, is represented by the earthquake of Herzogenrath; Type II., in which both zones are pretty equally distinct, and the depth is rather considerable, by the earthquake of Central Germany. There may exist a Type III. with a very deep centre, or with small intensity and moderate depth, for which the point of inflexion of the hodograph falls outside the region when the earthquake is perceptible, and where, consequently, the hodograph is convex throughout. Amongst the earthquakes so far studied, for which the mean velocity has been calculated, those with small velocities, which generally have a merely local character, may safely be regarded as belonging to the first type.

THE TOTAL SOLAR ECLIPSE OF AUGUST 8, 1896.[1]

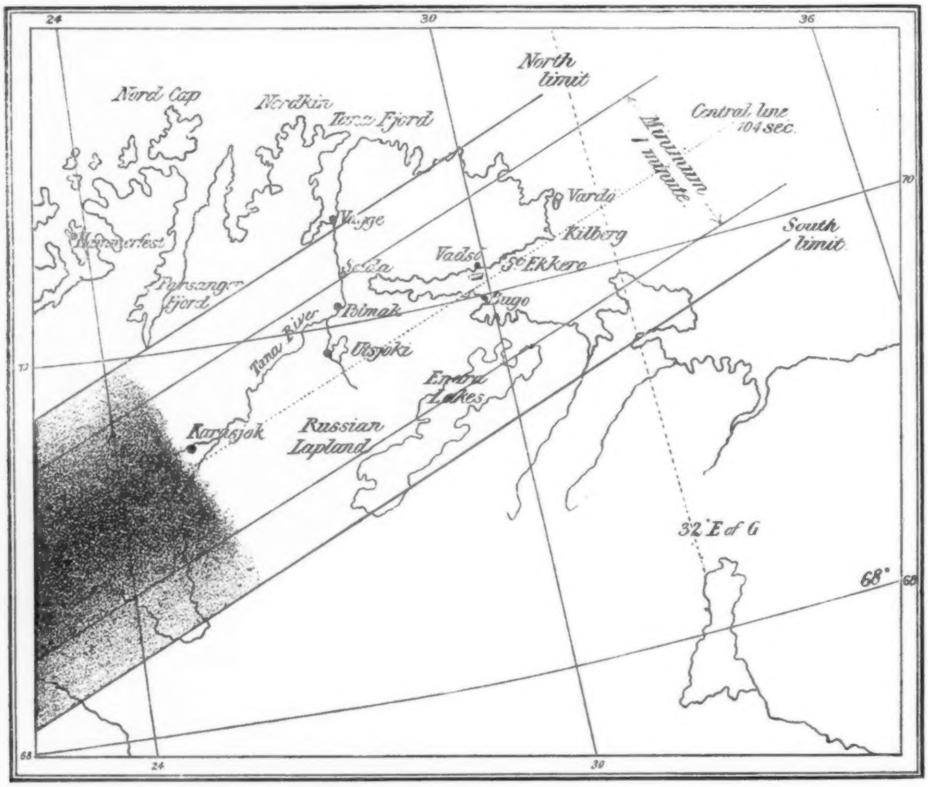
IT having come to my knowledge that some doubts had arisen as to the suitability of Norway as a post of observation for the total eclipse of the sun in 1896, and having had both experience in total eclipse expeditions and of travelling in Norway, I determined to make a special tour of observation both to the west coast, and also to Finmarken, Lapland, and the Russian frontier on the east coast.

In selecting stations in such an exceptional country as Norway, many points must be considered that do not apply to most places; thus it is not enough to know that A is twenty miles from B without also knowing how many fjords lie between, how many peaks three or four thousand feet in height, how many glaciers, and how far they are crevassed, if the mountains are passable, and if so what weight besides himself a man can carry up. Those people who have visited Norway, and the more so those who have travelled in the interior and north of the country, are surprised at the almost impossibility of moving at all except by the fjords and certain made roads. These generally may be said to extend as far north as Trondhjem, latitude 63° 26′; longitude 10° 30′ about. After that, on the north and north-east coast and Russian frontier, roads are the feeblest tracks generally, and the fjord communication only of a special character; the population, except at such places as Tromso, Hammerfest, Vardo, and Vadso, is very scanty, and chiefly confined to the sea coast, and in the latter case subject to consider-

[1] Abridged from a paper read before the Royal Astronomical Society, by Col. A. Burton-Brown (*Monthly Notices*, R.A.S., vol. lv. No. 3).

able variation, according to the season of the year and the nature of the fishing. How far these points would affect an astronomical expedition will be seen later on.

From Trondhjem we take a north-north-east course, passing Torghatten, an island of about 800 feet in height, and shortly after pass the island of Donnæso, at the bottom of the map on the west side, which will be on the border of the shadow during totality; steaming north to Tommen Öe, which will have about one minute totality, a careful search for stations is kept. Lüröe is too low, Oxtinden promontory too much inland. Hestmandöe, well within the 85″ line, and the island of Tränen, near the 12th parallel, are too far west, though the central line passes close by; but having a longitude of only 12° 5′, the sun is rather low. The height of ground is 3710 feet. Röd Löven also, somewhat further eastward, and Bolgen, a peaked island, close to the central line, are rather too far westward; passing Omnæs Oe and Melöe, crossing the central line, we next miles direct north of the central line, where there is a hill to the north-east of the town. Excellent accommodation can be had there, and a telegraph station exists. Further north is the island of Lande Gode, within the 85″ limit. The islands of Lundo and Engelo are on the northerly limit of the shadow of totality, and therefore useless. If Bödo were occupied as headquarters, with a steam launch at the disposal of the party, and Sandhornet, Arnöe, Fleina, Kunnen, or the lighthouse rock of Slöt, Bolgen, Röd Löven, or Hestmando as detached stations, a valuable area would be covered, although these positions have the astronomical disadvantage of the sun's altitude being as low as $6\frac{5}{8}°$ to $7\frac{3}{4}°$, and the duration of totality but little over $1\frac{1}{4}$ to $1\frac{1}{2}$ minutes. Yet, owing to the stillness of the atmosphere on the west coast of Norway at that time, and the general freedom from clouds on the horizon, some good results should be obtained. The further fact of the corona being seen through a considerable thickness of watery vapour would have both a spectroscopic and photographic



round the promontory of Kunnen, two or three miles above the central line, a rather inaccessible position of some 2000 feet high. The lighthouse island of Slöt is, however, in all respects a desirable position, and Kunnen has a telegraph station—a valuable adjunct. Proceeding east-north-east the Island of Fuglöe is passed, 2300 feet high, steep and rugged; and $1\frac{1}{2}$ miles further north, Fleina, about 400 to 1000 feet—a good position. On the right is the island of Sandhorn, with the Sandhornet Mountain of 3300 feet—a rare position for mountaineers, only about five or six miles above the central line, with a minute and a half totality, and the sun an altitude of $7\frac{1}{2}°$ about, the longitude being six or seven minutes over 14°. This position gives an uninterrupted view all round. For non-climbers the Arnöe Islands on the west, where there are fishing stations, would afford an almost equally good position. These places are in easy communication with Bödo, the latitude of which is 67° 17′, and longitude 14° 24′ about, and which is ten interest in comparison with similar observations taken at about double the altitude on the east coast. Although several island could be used as sites for stations, experience has shown some to be better adapted for many reasons than others. Thus Fuglöe is steep and rugged, and has no advantages over Fleina except height; also Omnæs Oe is a troublesome place; Kunnen is an almost inaccessible promontory of chert and granite rocks; the island of Slöt has a good anchorage for boats, and the sun would not be masked by Kunnen during totality. Here also is a telegraph station.

Leaving the west-coast positions, we reach Tromso, and steaming up to Hammerfest, signs of civilisation die fitfully away. After rounding the North Cape, 71° 10′ 40″ N., and about 26° E. and steering east, the Nordkin is passed, and from there in a south-east direction we travel along a bleak, inhospitable shore of quartzite rocks to Vardo, on an island; this is about the north-east corner of Norway, a good-sized whaling station, and a fort,

with some second-class hotel accommodation. This place was used as an observing station in 1769 by the Austrian Hell for the transit of Venus; and, being less than twelve miles directly north of the central line of shadow, might be advantageously occupied. It forms the most easterly station; being in longitude 31° 8′, and latitude about 70° 22′, it would have a duration of totality of over 1m. 31s., and the sun's altitude will be about 14½°. It is easily accessible, no high ground obstructs the view, and provisions and labour are to be had. Passing south down a dreary coast of quartzite rocks and Silurian slates, we come to Kilberg, about ten miles south, and two miles inland. There is a hill about 500 feet high, but although this would be only five or six miles north of the central line, it is not in other respects a desirable station. Steaming south-south-west we pass Store Ekkero, a promontory some twenty-five or thirty miles west of Vadso, which appears to have all the attributes of a good station, provided accommodation can be arranged for: the central line of totality passes over the southern point, and there is a free view to the south-south-east and east-north-east, the sun's azimuth at the local time of 18h. being 97° south towards east, and the duration of totality a maximum—viz. over 1m. 41s., the sun's altitude about 14¼°. Passing on to Vadsö, the town of the Finmarken district, there are several hills, two or three hundred feet, easily accessible, and in all respects suitable for observing stations within three or four miles; indeed, Vadsö should be looked upon as the headquarters of an East Norway expedition. The local time of totality here would be 17h. 57m. 46s., and duration about 1m. 35s. All the aforementioned places are in telegraphic communication with most parts of Norway during the fishing season, and no doubt arrangements could be made for keeping the offices open as late as August 8. The temperature at Vadso is remarkably high, probably between 50° and 60° F. in August, and there is every chance of fine weather at that time.

Crossing the Varanger Fjord we come to Bugö, a Lapp fishing station, and within a mile and a half of the central line; the longitude is about 29° 50′, and latitude 69° 58′. There the duration would be about 1m. 40s., with nothing to obstruct the view; frequent communication could be had with Vadsö at certain times of the day; heliograph signals might be transmitted; there are several sites hereabouts, but one in particular desirable. The Bugönæsfjeld I will leave to those who like to be in the clouds! So little is known of it that every map shows it in a different position; but if intrepid mountaineers are fond of carrying half-hundred-weights up mountains, there is no reason why they should not have the satisfaction they desire, but they will find no one to do it for them.

In order to distribute the parties and multiply the chances of success, one party might proceed from Vadsö to Seida, on the Tana River. This station is a good one for all points except the length of totality, which is only about 1m. 12s., and has the sun at an altitude of about 13½°. Polmâk, some twelve miles due south (reached by poling up the river), is not so easily got at, but astronomically better situated, and south-east of it, about five miles, is a mountain over 1000 feet high. About forty or fifty miles further up this river, in a south-west direction, is Utsjoki, a place also that might be advantageously occupied in the Russian Lapland. The duration of totality there would be about 1m. 26s., and the sun's altitude about 13°; both at Polmâk and Utsjoki camp equipage would have to be taken. Both are in telegraphic communication with Vadso and Vagge, the latter place being at the mouth of the Tana Fjord. Karasjok is astronomically a good place, within four miles of central line, the sun's altitude being about 12¼° and duration of totality over 1⅓m. Much, of course, will depend on the number of observers it is proposed to send out, their powers of endurance, and knowledge of Lappish, Russian, and Norwegian for the east coast expeditions (except at Vardo).

To the information which Colonel Burton-Brown has brought together, we may add that the Orient Steam Navigation Company propose to send one of their large steamships to Vadsö, for the purpose of enabling observations to be made of the eclipse. The steamer will leave London on July 21, and, after calling at Odde, Bergen, Naes, Molde, Trondhjem, Hammerfest, and North Cape, will arrive at Vadsö on August 3. It will leave a week later, and will arrive in London on August 17. (Full particulars of this journey will be found in our advertisement columns.)

We are informed by Messrs. Cook and Son that the Bergenske Steamship Company have consented, subject to certain conditions, to send one of their best steamers from Bergen and Trondhjem to Vardo and back, for the purpose of enabling persons interested in astronomy to view the eclipse. It is proposed that the steamer shall leave Bergen on July 31, calling at Trondhjem two days later, reaching Vardo on August 8, and remaining until 4 p.m. on August 9, returning to Trondhjem August 13, and Bergen August 15. The steamer will call at all the usual places visited by the tourist steamers between Bergen and the North Cape.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. Herman, of Trinity College, is appointed Chairman of the Examiners for the Mathematical Tripos.

The University Lecturer in Geography (Mr. H. Yule Oldham) announces a course of lectures on the Elements of Physical Geography during the present term. The Royal Geographical Society's Studentship of £100 will be awarded at Easter. Candidates must be members of the University who have attended the courses of the University Lectures.

The Council of the Senate recommend that the University of Allahabad be adopted as an affiliated University on terms corresponding to those in force for the University of Calcutta.

The report of the Syndicate on the Higher Local Examinations shows that good results have been attained in the scientific subjects. The new laboratory examination appears to work well, and has had a wholesome effect on the candidates' training.

Mr. W. C. D. Whetham and Mr. J. W. Capstick have been recognised as Teachers of Physics, and Mr. R. H. Adie as a Teacher of Chemistry, for medical degrees.

Among the freshmen who have matriculated this term, there are over 150 students of medicine.

SLOWLY, but surely, the system of paying teachers of elementary science according to the examinational successes earned by their students—in other words, according to their ability to cram young students with a large assortment of scientific facts, dogmatically stated and imperfectly understood—is giving way to one more calculated to create and foster a desire for natural knowledge. Within the past few days a Minute has been issued to schools under the Department of Science and Art, stating that the Lords of the Committee of Council on Education have decided to try the experiment of making grants for instruction in science and art depend partly upon the attendance of the student and partly on payments on results as tested by examination. The Committees of Science and Art Schools and Classes which have been in the receipt of grants from the Department for two consecutive years, or which are conducted by a local authority under the Technical Instruction Act 1889, or the Technical Schools (Scotland) Act 1887, will be allowed to elect to receive their grants on the scheme under which the payments on results will be one-half those on the present scale, while attendance grants will take the place of the other half, provided that the Inspector of the Department reports that the teaching and equipment of the school are thoroughly satisfactory, and that the class or classes are not too large for instruction by the staff of teachers. The attendance grant will be 1*d.* for each attendance of at least an hour's duration in a day science class, and 2*d.* in a night science class, and of 3*d.* for each attendance of one and a half hours' duration given to practical work in chemistry, physics, metallurgy, or biology, in a properly equipped laboratory. Applications to receive grants under the new Minute must be received before December 1, 1896, and in subsequent years before November 1. But the sanction to be so treated may be withdrawn at any time should it appear from the results of the examination in May, or from the reports of the Inspectors, that the instruction is not efficient; and no school can receive grants partly under the new Minute and partly under the ordinary scale of payments on results. Organised science schools are exempted from these attendance grants; nor can the grants be claimed on behalf of students who are on the register of an elementary school. The principle of recognising attendance at classes as one of the tests of the efficiency of a school has common sense at the back of it, and it should do something to reduce the baneful influence of the examination fiend upon elementary scientific education.

DR. A. ROTHPLETZ has been appointed Extraordinary Professor of Geology and Palæontology in the University of Munich; Dr. Ernst Lecher, Professor of Physics in Innsbruck University, has been nominated to succeed Prof. Machs at Prague; Dr. F. Marés has been made Ordinary Professor of Physiology in the Bohemian University at Prague; and Dr. J. E. Humphrey has been appointed Lecturer in Botany at the Johns Hopkins University, Baltimore.

THE Calendar of the University College, North Wales, for the year 1895-96, has been received. The physical, chemical, and biological laboratories (plans of which are given in the Calendar) now cover an extensive area. Under Prof. Andrew Gray, the physical department has greatly developed; and the appliances and electrical installation with which it is equipped enable the College to offer a complete course of instruction in all branches of electro-technical education.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 14.—M. Janssen in the chair.—The decease of Baron Larrey, free member, was announced from the chair. He died on October 8. M. Émile Blanchard pointed out the great influence of the deceased in modern surgery.—The Prince of Monaco has sent to the Academy No. ix. of his publications concerning the scientific work done on his yacht: a contribution to the study of the Cephalopods of the North Atlantic, by M. Louis Joubin.—On a mechanical amplification of the horizontal component of the earth's rotation, by M. Jules Andrade.—On a hydraulic apparatus to show the movement of rotation of the earth, by M. Aug. Coret.—M. Aug. Fabre, in a memoir on "Integration of the equation to the derived partials of the first order, of a function x with n independent variables $x_1, x_2, x_3, \ldots, x_n$" gives a quick new method of arriving at the general integral of an equation $\mu(x, x_1, x_2, \ldots, x_n, \rho_1, \rho_2, \ldots, \rho_n) = 0$.—M. J. Janssen, in the name of the Bureau des Longitudes, presented the 1898 volume of "Connaissance des Temps." There has been added to the tables concerning the satellites of planets, a table giving the elements for the calculation of the position of Mars' satellites at any given moment. In the ephemerides of the fundamental stars, the brightness of those above the first magnitude has been given, taking Aldebaran as unit.—The Perpetual Secretary announced to be printed in the Correspondence, "Theorie der endlichen Gruppen von eindeutigen Transformationen in der Ebene," by M. S. Kantor.—On a class of linear equations to the derived partials, by M. H. von Koch.—On the surfaces of which the lines of curvature form a network with equal tangential invariants, by M. A. Thybaut.—On the double elliptic refraction and the tetrarefringence of quartz near its axis, by M. G. Quesneville.—On the estimation of argon, by M. Th. Schlœsing. An apparatus with circulating mercury pump is described, which allows of the absorption of nitrogen and measurement of the residual argon. The whole arrangement is a modified form of Ramsay's apparatus for isolating argon.—On the action of hydrochloric acid on copper, by M. R. Engel. Copper decomposes a saturated solution of hydrogen chloride at 15° C., with liberation of hydrogen. This interaction does not occur if the concentration be less than that shown by the formula $HCl.10H_2O$. The presence of cuprous chloride retards the reaction greatly.—Action of potash and potassium ethoxide on benzoquinone, by M. Ch. Astre.—On combinations of antipyrine with the diphenols, influence of the respective positions of the hydroxyl groups, by MM. G. Patein and E. Dufau. Pyrocatechol, resorcinol, and quinol (hydroquinone) behave differently with regard to antipyrine; the ortho- and para-diphenols combine with two molecular proportions, the meta- with one. The combination occurs through one of the nitrogen atoms and the phenolic hydroxyl, which loses this property when its hydrogen is replaced by a metal or radical.—Experiments on the reducing power of pure yeasts, means of measuring it, by M. Nastukoff.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Elements of the Mathematical Theory of Electricity and Magnetism: Prof. J. J. Thomson (Cambridge University Press).—Elementary Physiology: Prof. J. R. A. Davis (Blackie).—A Directory of Science, Art and Technical Colleges, Schools and Teachers in the United Kingdom: R. S. Lineham (Chapman and Hall).—A Manual of Physiology: Dr. G. N. Stewart (Baillière).—Movement: F. J. Marey, translated by E. Pritchard (Heinemann).—Frail Children of the Air: S. H. Scudder (Boston, Houghton).—Darwin and after Darwin: Dr. G. J. Romanes, ii. (Longmans).—Among Rhode Island Wild Flowers: Prof. W. W. Bailey (Providence, R.I., Preston).—Pagan Ireland: W. G. Wood-Martin (Longmans).—First Steps in Egyptian: Dr. E. A. W. Budge (K Paul).—Birdcraft: M. O. Wright (Macmillan).—Fishes, Living and Fossil: Dr. B. Dean (Macmillan).—Science and Art Drawing: J. H. Spanton (Macmillan).—Great Astronomers: Sir R. S. Ball (Isbister).—Elektrophysiologie: Prof. W. Biedermann, Zweite Abthg. (Jena, Fischer).—Protobasidiomyceten: A. Möller (Jena, Fischer).—The Tallerman-Sheffield Patent Localised Hot-Air Bath (Baillière).—University College of North Wales, Bangor, Calendar for the Year 1895-6 (Manchester, Cornish).—Atlas d'Ostéologie: Prof. C. Debierre (Paris, Alcan).—Evolution and Effort: E. Kelly (Macmillan).—A Handbook of British Lepidoptera: E. Meyrick (Macmillan).—Surface Currents of the Great Lakes: M. W. Harrington, revised edition (Washington).—Annuario p.p. Observatorio do Rio de Janeiro, 1895 (Rio de Janeiro).—U.S. Geological Survey Report, 1892-93, 2 parts (Washington).

PAMPHLETS.—Neue Versuche zum Saison-Dimorphismus der Schmetterlinge: Dr. A. Weismann (Jena, Fischer).—Neue Gedanken zur Vererbungsfrage: Dr. A. Weismann (Jena, Fischer).—Cavendish Lecture on Dreamy Mental States: Sir J. Crichton-Browne (Baillière).—The People's Stonehenge: J. J Cole (Sutton).—Iron and Steel Institute: Presidential Address: Sir D. Dale; Metal Mixers: A. Cooper; The Effect of Arsenic on Steel: J. E. Stead; The Mines of Elba: H. Scott; On the Manufacture of Steel Projectiles in Russia: S. Kern; Ternary Alloys of Iron with Chromium, Molybdenum, and Tungsten: J. S. de Benneville (Victoria Street).—The Siouan Tribes of the East: J. Mooney (Washington).—Archæologic Investigations in James and Potomac Valleys: G. Fowke (Washington).—Chinook Texts: F. Boas (Washington).

SERIALS.—Proceedings and Transactions of the Queensland Branch of the Royal Geographical Society of Australasia, Vol. x. (Brisbane).—Quarterly Review, October (Murray).—Journal of Anatomy and Physiology, October (Griffin).—Contributions from the U.S. National Herbarium, Vol. 3, No. 3 (Washington).—Jahrbuch der k.k. Geologischen Reichsanstalt, xlv. Band, 1 Heft (Wien).—Societa Reale di Napoli, atti della Reale Accademia delle Scienze Fisiche e Matematiche, serie second, Vol. vii. Napoli).—American Journal of Psychology, Vol. vii. No. 1 (Worcester, Mass.)—Ethnologisches Notizblatt, Heft 2 (Williams and Norgate).—English Illustrated Magazine, November (198 Strand).—Transactions of the Academy of Science of St. Louis, Vol. vi. No. 18, Vol. vii. Nos. 1, 2, 3 (St. Louis, Mo.).—Transactions of the Wagner Free Institute of Science of Philadelphia, Vol. 3, Part 3 (Philadelphia).—Proceedings of the American Philosophical Society, January, 1895 (Philadelphia).—Proceedings of the Academy of Natural Sciences of Philadelphia, 1895, Part 1 (Philadelphia).

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